

MAN AND A HALF

ESSAYS IN
PACIFIC
ANTHROPOLOGY
AND
ETHNOBIOLOGY
IN HONOUR OF
RALPH BULMER



EDITED BY ANDREW PAWLEY



MEMOIR No. 48

MAN AND A HALF

***ESSAYS IN PACIFIC ANTHROPOLOGY
AND ETHNOBIOLOGY
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edited by

ANDREW PAWLEY

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EDITORIAL NOTE

In June 1988, when Ralph Bulmer was already mortally ill, invitations were sent to over 100 friends and colleagues of his, inviting them to contribute to an extraordinary Festschrift. It was extraordinary because, knowing that Ralph probably did not have long to live, I asked each person to submit a paper within six weeks and to donate \$100 towards publication costs, with the aim of presenting a modest book to Ralph before the end of the year. Because of this stringent deadline I did not expect to receive more than a handful of papers.

The response was astonishing. Almost 90 people immediately sent in titles (a few jointly) and within five weeks drafts of more than 20 papers had arrived. Ralph had read the provisional table of contents before he died on July 18, 1988. After his death it became necessary to rethink editing and publication plans. Because of the large number of papers a length limit was imposed, authors who had not already submitted their papers were sent a style guide and all essays were sent to appropriate readers for critical assessment. The final product consists of 81 papers by 90 authors. In the end, publication was in the hands of the Polynesian Society, in Auckland, production being undertaken in Canberra.

Ralph would have been delighted with the quality and range of the papers in the volume. Part I consists of several memoirs. The remaining Parts, II-VI, represent the considerable range of disciplines which he contributed to – chiefly, social anthropology, ethnobotany and ethnozoology, archaeology, oral history and linguistics – as well as issues of social change and modernisation in Pacific societies with which Ralph was much concerned.

Thanks are due to the many people who contributed to the project. To the editorial committee of Bruce Biggs, Nancy Bowers, Roger Green, Chris Healey, Judith Huntsman and Richard Moyle, who gave initial encouragement and advice. To Lena Bulmer, who supplied addresses and contacted several potential contributors. To the authors for providing a fine collection of papers. To all those scholars who acted as anonymous readers and critics. The title, *Man and a Half*, was suggested by Bruce Biggs who averred that Ralph was sometimes called by this name when they were together in New Guinea. Marlous Terwiel drew the frontispiece, 'Ralph Bulmer in Birdwatching Mode', and Chris Healey kindly allowed us to use his cassowary drawing on the cover and dividing pages. During 1989 the papers were entered on word processor by Dorothy Brown and Inger Miller at the Department of Anthropology at the University of Auckland. After my shift to the Research School of Pacific Studies, ANU, in February 1990, technical problems required a good many of the manuscripts to be retyped by Lorraine O'Brien, Anne Rees and Jeanette Coombes. Anne Rees recreated some tricky diagrams and tables on word processor; the cartography unit at the Research School prepared or touched up several of the illustrations and Aletta Biersack gave editorial assistance. Other debts of gratitude for information about Ralph's life are recorded, more appropriately, in footnote 1, on page 19.

Above all, I am indebted to Lois Carrington, who not only did most of the sub-editing and checked and revised all the bibliographies but used her skills in book design to transform the roughly formatted papers into handsome camera-ready copy. Tengkyu tru, Lois.

Finally, thanks are due to all those friends of Ralph whose contributions to the Bulmer Fund provided a financial base for publication.

Sadly, I must record that two of our authors died while this volume was being prepared. We lost Garth Rogers in March 1989 and Georgeda Buchbinder in February 1991.

Andrew Pawley
18 October 1991

PART I

MEMOIRS



NOTES ON A LARGE SMALL GAME HUNTER

Andrew Pawley
Australian National University

On arrival one was greeted by the sight of an enormous pair of shorts and short-sleeved shirt wafting in the breeze. The Melanesians must have imagined this surely was the outer soul of a giant, sloughed off only moments before. We were on the campus of the young University of Papua New Guinea, with its clothes-hoists and prefab houses on stilts. But once you passed through the door you discovered a unique kind of hospitality, which Ralph Bulmer offered completely naturally to visitors among the spirals of smoke emanating from an everlasting light-tobacco cigarette. In French academic circles one is quite unaccustomed to such a mixture of professional gravity and good humour, fathomless erudition and disarming simplicity which one took away from every meeting with him (Michel Panoff 1988:89: translated from the French).

I. THE BARE FACTS

Daunted by the prospect of having to review 60 years of Ralph Bulmer's life and times, I cunningly contrived for others to write about various phases and facets of them. The resulting essays form the bulk of Part I – beautiful pieces by Ellaine Mabbutt about Ralph's salad days at Cambridge, by Helga Griffin about his five years as Professor in the golden age of the University of Papua New Guinea, by Saem Majnep about how the Kalam saw him and by George Marcus about the Bulmer-Majnep collaboration – with Alice Bulmer's bibliography rounding out the section.

But sitting down to write this piece I found I had outwitted myself. Much of the best ground had already been covered by those who have written short memoirs, giving their personal views of Ralph in a specific context. And scattered through Parts II-VI you will find perceptive remarks about Ralph's contributions to particular domains of scholarship by other authors more qualified than I (see, for example, the papers by Berlin, Susan Bulmer, Douglas, Ellen, Ernst, Feld, Fox, Golson, Groves, Hays, Hunn, Helliwell, Jackson, Marcus, Meggitt, Mountain, O'Collins, Pouwer, Riebe, Yen and Young, who between them touch on his roles as ethnographer, ethnobiologist, prehistorian, fieldworker, theorist, teacher, colleague, etc.)

I too will recount memories. But what is left to me, principally, is the drier project of chronicling the main events of Ralph's life and the difficult job of distilling from the whole an impression of what made him tick, striving for insights into his character, his motives, his creative processes. For these are the things we always want to know about a person: What was he like? Why did he do what he did? Why was he received as he was?¹

The bare facts are as follows. Ralph (pronounced, traditional English-style, as 'Rafe') Neville Hermon Bulmer was born in the small English cathedral town of Hereford, near the Welsh border, on 3 April 1928, the eldest of three children. From the age of 11 to 18 he attended a boarding school in Sussex. After army service in 1947-49, he went to Cambridge where he read anthropology and archaeology. By 1953 he had married Ellaine Bruce, graduated with first class honours and lost most of his hair.

The following year Ralph took up a doctoral scholarship at the Australian National University in Canberra. Between December 1954 and the end of 1956 he did some 13 months' fieldwork among the Kyaka Enga of the Baiyer River in what was then Western Highlands District of the Territory of Papua and New Guinea, returning there for four months in 1959. This work was the basis for his thesis on Kyaka leadership and politics and for several published papers.

By 1958 he had lost a wife but gained a job in New Zealand – a lectureship in social anthropology at the University of Auckland.² In 1959 he married Susan ('Sue') Hirsh. They had a daughter, Alice, in 1961, and two sons, David and Kenneth, in 1962 and 1964. Soon after going to Auckland Bulmer began a long-term project among the Kalam, a recently-contacted 'pygmy' people of the Schrader Ranges, Madang Province, Papua New Guinea, focusing on their social organisation and perception and use of the natural environment. Over the next 25 years he made 14 field trips to the Kalam area, totalling two and a half years, and fostered research there by several other scholars in a range of disciplines. On five occasions he brought Kalam collaborators to Auckland for periods of two to six months. His findings on Kalam ethnobiology and cosmology were set out in a series of papers published in the late '60s and the '70s. In 1966 he made the first of three visits to Paris, where he established good rapport with a number of French anthropologists and biologists.

In 1968 he and his family moved to Port Moresby, where he served for five years as Foundation Professor of Anthropology at the University of Papua New Guinea. In 1974 he returned to New Zealand to take up the Chair in Social Anthropology at the University of Auckland.

In the 1970s Ralph began experimenting with a new kind of ethnography, with a Kalam hunter and naturalist, Saem Majnep, as the primary author and Ralph serving as translator, editor and commentator. Their first book, *Birds of My Kalam Country*, appeared in 1977. The Kalam text and English translations for a bilingual second book (English title: *Animals the Ancestors Hunted*) were substantially drafted by 1980 although the revision process continued through the 1980s. Ralph's marriage to Sue ended in 1980. He married Lena Lane in 1983 and their son, Richard, was born the next year. Work on a third book with Saem Majnep, *Kalam Plant Lore*, was in progress when cancer struck Ralph early in 1988. He died in Auckland on July 18 of that year.

II. RALPH BULMER'S PLACE IN ANTHROPOLOGY

It was only when Ralph Bulmer was dying that we, his colleagues at Auckland, realised his true standing in the field of anthropology. How widely he was respected, even revered, was shown by the response of scores of scholars from around the world to news that he was seriously ill and to the call for a collection of papers in his honour. I was taken aback by the sheer volume and emotional intensity of the responses.

Up till then I had thought of Ralph as a man with exceptionally wide-ranging scholarly interests and expertise who had by the age of 30 devised a unique long-term research agenda – but in a rather specialised domain, one of limited concern to most anthropologists. I knew that over the next decade he had vigorously pursued this agenda, and had written a good deal, including one or two theoretical papers that had achieved a measure of international recognition. But in the last 20 years of his life he took on other commitments which cut savagely into his research time. From 1967 on he was invariably either head of a large Department of Anthropology or of its largest division, or both, carried a demanding teaching load and was active in the affairs of his university, discipline and community – and consequently did not finish the second and third books of his trilogy with Saem Majnep. Furthermore, he spent most of his career at Auckland, admittedly one of the headquarters of Pacific anthropology but in terms of the traffic and politics of ideas in theoretical anthropology a provincial backwater. And while he enjoyed being in a department made up of diverse disciplines and took a keen interest in his colleagues' researches, there was no one among us who was expert in or who cared deeply about his speciality. Only occasionally did he venture onto the international lecture circuit to peddle his ideas in person to a wider audience.

However, the impact of Ralph's work was clearly much greater than most of us in his own Department realised. In Festschriften it is of course customary to praise, but I have read few books where so many contributors, from so many disciplines, have so clearly expressed their debts for intellectual inspiration and personal encouragement to the scholar they are honouring. Furthermore, there is a remarkable consistency between them in the kinds of things they remark on.

Bulmer was admired not because he published extensively on the traditional issues of social anthropology – he did not – nor even because he broke new ground – which he did – but because he did certain things so well, perhaps better than anyone had done before. Of his work in ethnobiology, Brent Berlin says simply: 'Bulmer, perhaps more than any other single individual, set the standards of field research, evidence and analysis that have led to modern ethnobiology as a discipline' (Berlin, this volume, p.65).

Eugene Hunn's opening remarks echo these sentiments and elaborate:

Ralph Bulmer set the standard for modern ethnozoological investigation much as Harold Conklin did for ethnobotany. Bulmer's investigations of Kalam ethnozoology and his penetrating reflections of the broader implications of his work are characterised both for the knowledge of his native collaborators and for the highest standards of Western science. Bulmer was able to appreciate at the same time the common foundation on which scientific and folk biologies are built and the subtle nuances of difference that grant to each culture a unique perspective on nature. It is this tension between the particular and the general that defines the anthropological stance among the sciences of humanity, and Bulmer's work is a powerful reminder of the necessity of balancing meticulous description against theoretical simplification (Hunn, this volume, 137).

It could be said of some scholars but never of Ralph that:

He who would study organic existence,
First drives out the soul with rigid persistence;
Then the parts in his hand he may hold and class
But the spiritual link is lost alas!

(from Bayard Taylor's translation of *Faust*)

That is to say, he listened to both Mephistopheles and Faust, the objective analytical observer and the sensitive interpreter, and in his syntheses he achieved a balance of the two.

In social anthropology at large it was Ralph's detailed analyses of Kalam belief and knowledge systems that had the widest impact. Chris Healey has this to say about his most famous paper:

Through his consummate ethnographic skills Ralph Bulmer left us an extraordinarily rich record and analysis of the natural history lore and symbolic structures of the Kalam . . . One of his most significant works was one of his earliest publications on the Kalam: 'Why Is the Cassowary Not a Bird?' (Bulmer 1967). This work, whose mildly whimsical title suggests something of his unassumingly innovative approach to Melanesian ethnography, helped set the stage for the more recent development of studies of Melanesian symbolic systems. It marked an important shift of emphasis in Ralph's own work, from a predominant concern with Kyaka Enga corporate political and economic structures . . . to the conceptual schemata of the Kalam . . . This change of focus anticipated a similar shift in Melanesian anthropology in general (Healey, this volume, 234).

The timing of the Kalam papers may have aided their reception. By the '60s and '70s theoretical social anthropology in the English-speaking world had rather lost its direction. Kinship and social structure, where the major analytic advances had been made in the last half-century, now seemed to those seeking new fields to conquer to be too well-traversed and confining. Several alternatives had gained a measure of popularity – among them structural treatments of belief systems and myth, where the French led the way, and linguistics-inspired structural analyses of terminologies, developed mainly in the USA. But stimulating though they were, a good many of the early studies done under these rubrics appeared to have the character of clever formal games, without a grounding in careful ethnography. While the papers on Kalam ethnobiology and cosmology touched on fashionable issues they also rested on ethnography that was four-square solid and this combination of virtues was welcome.

Ralph wrote as follows about the natural biases that shaped his ethnography:

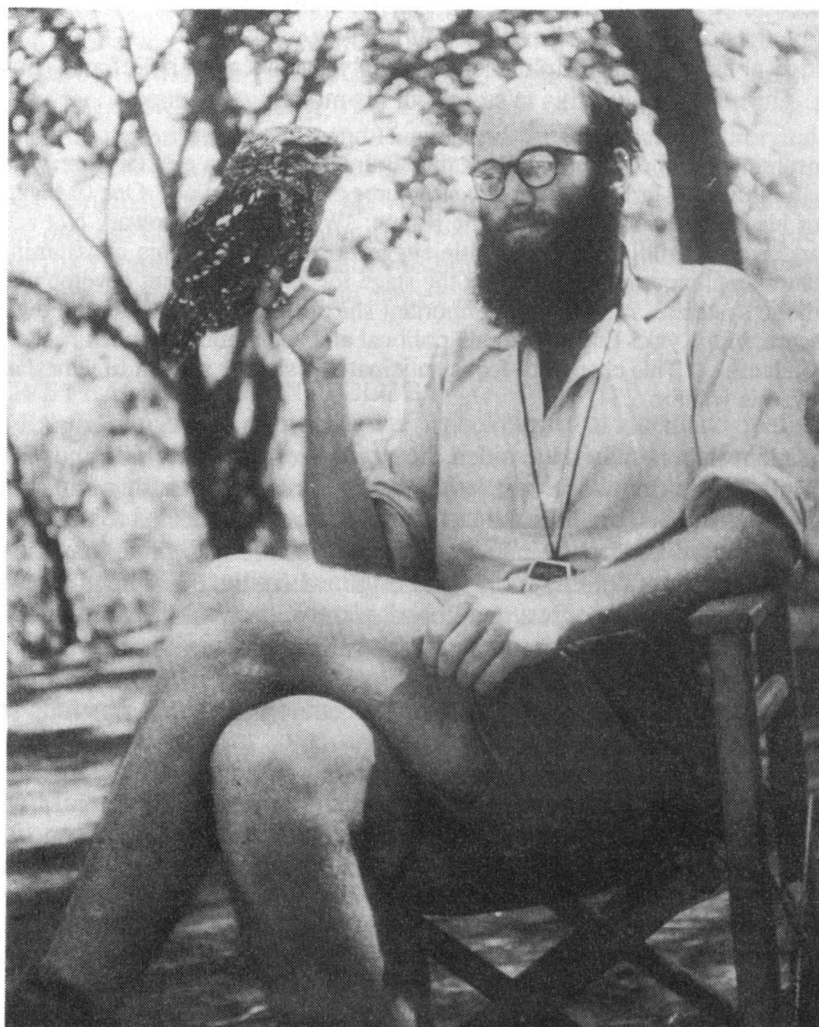
I have no personal inclination towards the kind of problem-oriented ethnography which records virtually nothing that is not directly relevant to the testing of hypotheses generated outside that particular study. My cast of mind leads me instead to pick on particular puzzles or curiosities arising within my own ethnographic data (sometimes quite trivial ones, such as why cassowaries are not classified as birds, why earthworms are believed to croak, and why one small lizard out of a series of nine placed in the same generic taxon is avoided and feared, whereas the others are regarded with equanimity . . .) and, in attempting to arrive at satisfying answers to these questions, to range as widely through my data as whatever concepts and theories I can draw on from my reading will allow. I am aware that I may appear to my readers as a kind of academic bricoleur, taking this piece of super-structure from Lévi-Strauss, that metaphorical connecting link from Mary Douglas, this rather rough piece of unworked argument from the ethologists – but I would hope nevertheless that regardless of the validity or otherwise of the *ad hoc* explanatory structures I assemble, I am also presenting ethnographic data which can be subjected to reanalysis by others (Bulmer 1974:94).

But the Bulmerian zone of influence extended beyond social anthropology and ethnobiology. This book resonates with the voices of Melanesia specialists from other fields – prehistory, linguistics, ethnohistory, education, social and political change and environmental conservation, especially – touched by Ralph's contributions both as a scholar and as a facilitator of others' research.

III. BECOMING A SMALL GAME HUNTER: THE FIRST STEPS

Ralph saw himself as a man from a modestly-placed middle-class family who by strokes of good luck was enabled to live the life, almost, of an old-fashioned gentleman-naturalist. He could readily imagine himself, in another age, as a country parson like Gilbert White (whose *Natural History of Selbourne*, I cannot help mentioning, is the fourth most reprinted book in the English language). But the boy from Hereford became an anthropologist, working first in what we used to call Lapland and later in central New Guinea, and a pioneer in ethnozoology. How did these things come to pass?

Ralph's sister, Rosemary Pownall, told me a good deal about his boyhood when she and her husband were in Auckland in 1988. Both Ralph's parents came from North Wales – his father, Kenneth Julian, from Holyhead, his mother, Dorothy Hughes, from Bangor. They settled in Hereford where Kenneth in due course became a bank manager and where their three children were born. Rosemary and John were, respectively, three and ten years younger than Ralph. The Bulmers lived in a suburban house on an estate about two miles from the town centre of Hereford. Climb over the back fence and you were in the fields. Ralph very early acquired a love of natural history from his father, who took him for walks and bird-nesting. Family tradition is that the first words he ever spoke were "'ook, 'obin!" ("look, robin!"). "Once while our mother was



Ralph with Frogmouth Owl

away," Rosemary told me, "he brought home a dead rabbit and was dissecting it on the kitchen table amidst all the washing up that hadn't been done." Years later, Ralph was to write an essay called 'Memoirs of a Small Game Hunter' (Bulmer 1974), and at one point in this he suggests that certain childhood interests are prerequisite to becoming an ethnobiologist:

I hope I am not being unduly introspective if I suggest that to be a committed ethnobiologist one has to be a collector, even a trophy-hunter by nature. Just as a host of amateur (and also, professional) naturalists . . . come to substitute the collection of observational records of animals or plants for the collections of birds' eggs or butterflies or pressed flowers of their childhood, so the ethnobiologist moves one stage further and derives deep satisfaction from the collection of animal and plant categories (Bulmer 1974:81).

At the age of 11 Ralph won a scholarship to Christ's Hospital, the well-known church school in Horsham, Sussex, which for centuries had educated able children from families of modest means and whose scholars wore mediaeval dress. Rosemary feels that, in Ralph's case, going to a private school helped him. He was head of his house and the experience of leadership at an early age gave him confidence. Rosemary said "When he came home on holidays he always had so many anecdotes to tell about things that happened at school that I couldn't bear to go out of the room when he was talking. I used to be spellbound."

Ralph and Rosemary made lifelong friendships with a lively, unconventional family who came to live next door: "They stirred us out of our middle-class Herefordian stodginess," Rosemary said. One of the girls, Yvonne, later married Ralph's best friend from Christ's Hospital, Christopher Houlder. Christopher shared Ralph's interest in bird-watching and archaeology. Archaeology was in Ralph's blood: his maternal grandfather was in this field and (as Susan Bulmer (this volume, 470, relates) Ralph virtually cut his archaeological teeth on the monuments of North Wales.

Ralph grew up to be a very large small game hunter, nearly 6' 6" (1.98cm) and over 17 stone (112 kg). His place in the school rugby team was, naturally, at lock forward. In his final year at school he wrote a remarkably mature little monograph on the Cuckoo and won a minor scholarship in modern languages to Clare College, Cambridge. As Ralph's talents in and dedication to languages never equalled those he had for natural history, one has to wonder at the narrow thinking behind a classical education that channelled into languages a boy so obviously born to be a naturalist. Ralph did not go straight to university. First he did army service, spending two and half years, from August 1947 to February 1949, in the educational corps. Rosemary tells a story about the time when he was made Acting Captain. "He looked a great deal older than he was and when, aged 20, he was sent to the Rhine to replace an officer several years older than himself, his CO said to him "Well, it's nice to have a mature chap at last! These young fellows don't know what they're doing."

IV. CAMBRIDGE

In the autumn of 1949 Ralph went to Cambridge to read anthropology and archaeology at Clare College. There are different stories about how he arrived at this choice of disciplines. His formal qualifications were in modern languages but secretly he wished he could do zoology. According to Rosemary he mentioned his plight to a man who then said, "Why don't you study human beings? They're the most interesting species of all." No doubt Ralph had this compromise in mind when he later wrote:

I have myself had no formal training in biology or any other natural science since the age of thirteen, but I have had a life-long interest in natural history, and especially ornithology. When I commenced my undergraduate training I was more conversant with the comparative behaviour of birds, both from personal observation and from direct and indirect contact with professional ornithologists and other students of animal behaviour, than I was with the comparative behaviour of man (Bulmer 1974:82-3).

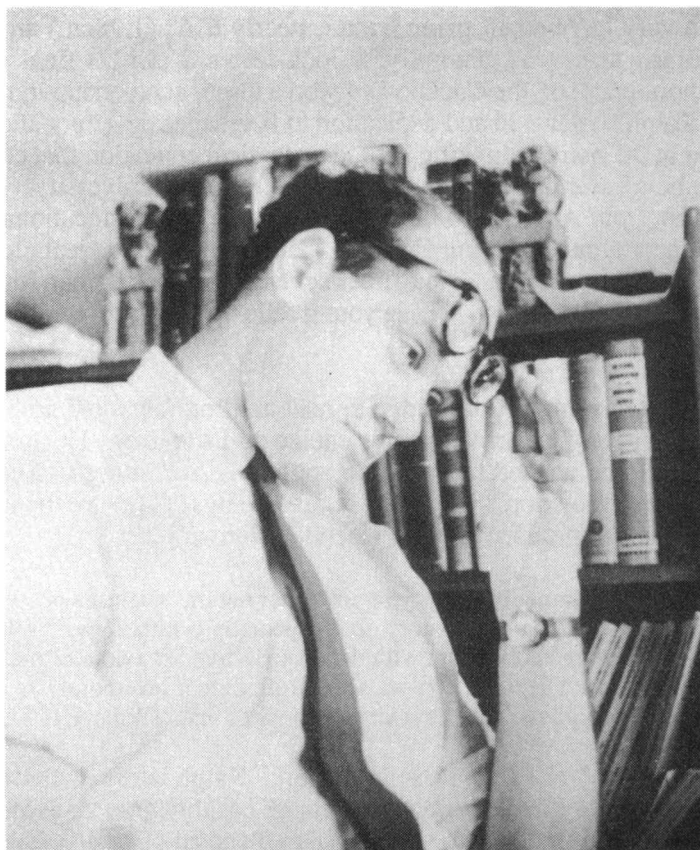
His tutor in archaeology was no less than Desmond Clark. Ralph later recalled how Clark had impressed upon him the value, for interpreting the archaeological record, of ethnographic evidence concerning the siting and construction of camps and the slaughter of animals and disposal of bone. Ralph became one of a small group of students doing research among the reindeer-herding Sami of northern Sweden and Norway, under the aegis of the formidable Ethel-John Lindgren (known by her students as E.J.) and her Sami husband, Mikel Utsi. In 1950 Ralph and another promising student, Ian Whitaker, went to the field on separate operations and Ralph spent some six months based in Sor-Varanger. He wrote half a dozen unpublished reports on aspects of Sami life, including reindeer herding, kinship and marriage, and hunting. His notes, reports and photographs (to be deposited at the Institute of Social Science, University of Tromsø) show him to be already an excellent ethnographer who enjoyed fieldwork immensely. E.J., a much respected but difficult mentor, was a perfectionist who evidently approved of and reinforced Ralph's innate habits of thoroughness in observation and recording.

However, the early '50s were the heyday of the new British structuralism which owed much to Radcliffe-Brown and whose main exemplars were the classic ethnographic monographs on African societies. At Cambridge there gathered some of the leading representatives of this school. Ralph's training in ethnographic theory at Cambridge was under the likes of Meyer Fortes (who arrived mid-way through Ralph's studies, to head the Department) and Jack Goody. Reo Fortune was the sole Melanesianist there. Edmund Leach arrived after Ralph's undergraduate years but was there when Ralph went back to Cambridge as a tutor in 1957. Ralph's interests in natural history were known to his teachers and evidently regarded as a harmless personal hobby, though years later we find Fortes' name among those who gave interested comments on one of the Kalam cosmology papers.

Marie Lawrence, a fellow student at Cambridge, has written of Ralph's circle of friends united by "an interest in anthropology and archaeology, natural history, films (especially Marx Brothers) and music. We shared a passion for Peter Pears and Benjamin Britten and we must have worn through Ralph's record of 'The foggy foggy dew . . .'. "In his final year at Cambridge he had a whirlwind romance with a member of this group, Ellaine Bruce. Ellaine (now Mabbutt) has written a delightful cameo piece recalling the Cambridge scene.

V. AUSTRALIA AND NEW GUINEA

E.J. Lindgren was said to be very displeased when Ralph decided to go to the other end of the world for his PhD research. Ralph enjoyed his Sami studies immensely but New Guinea had become a magnet attracting bright young British and North American graduates who a few years earlier might have gone to Africa.³ At the Australian National University the newly-established Department of Anthropology had become almost a colony of British social anthropology, with Nadel (an Austrian) as its first governor-general. The ANU offered generous scholarships and support for fieldwork in the Highlands of what was then Australian New



A contemplative Ralph at the Australian National University in 1957 (R.M. Glasse)

Guinea. And perhaps the young couple's decision to go there had something to do with escaping the stuffiness of British academic life and certain family pressures.

So it was that in 1954 Ralph joined the rush to the new frontier. Ralph's contemporaries at the ANU included several of the scholars who did the first modern ethnography in the Highlands: Bob Glasse, Marie Reay and Dick Salisbury; and at Sydney, Mervyn Meggitt and D'Arcy Ryan. He chose to work with a fringe Enga group known as the Kyaka, of the Baiyer Valley in the northern foothills of Mt Hagen. He and Ellaine went to the field in December 1954 and spent much of 1955 and part of 1956 based at Yarramanda, 1550m above sea level. They visited some of Ralph's ANU fellow students in the field and each mentions that, among other things, Ralph took pains to sort out their bird identifications for them.

At this stage, Bulmer the naturalist remained a separate identity from Bulmer the anthropologist:

In my first two periods of fieldwork in New Guinea, . . . I spent 17 months with the Kyaka . . . The official focus of this study was on social structure and leadership, but I spent a great deal of time collecting fauna and getting what information I could about Kyaka knowledge of plants and animals. The Kyaka taught me far more than any of the publications then available to me about the fauna . . . of the New Guinea Highlands. But though I obtained much information, I cannot say that my investigations of Kyaka zoological knowledge were at all systematic and rigorous . . . Also I was at that stage able to get little help from natural scientists in identification of specimens other than birds and fishes. My first publication, a naively written popular account of Kyaka bird lore . . . has rightly been criticised . . . though I believe that it is in essentials and most details accurate (Bulmer 1974:83).

Ralph also collected artefacts and noted potential archaeological sites, knowledge which other scholars were later to take advantage of. His last lengthy visit to the Baiyer, of which I will speak below, was for four months at the end of 1959. In later years he kept in contact with Kyaka friends and also with members of the Baptist Mission at Yarramanda, whose medical work he held in high regard. Ralph's thesis on Kyaka leadership and social structure was supervised first by Nadel and then, after the latter's sudden death in 1956, by Derek Freeman. Though still highly regarded by others working in this field, the thesis was never published; however, Ralph did publish a number of papers on Kyaka politics, religion and exchange systems, as well as one on Kyaka ornithology.

VI. NEW ZEALAND

In January 1957 Ralph crossed the Tasman to attend a conference in Dunedin. There he met two members of the Department of Anthropology at Auckland, Jack Golson (a fellow student at Cambridge) and Bill Geddes, who in turn recommended him to their Head of Department, Ralph Piddington, for a lectureship in social anthropology to become available the following year. Now separated from Ellaine, he returned to England at the end of 1957 and tutored at Cambridge for a few months, before taking up the position in Auckland in April 1958.

Among the 120 Stage I Anthropology students present when Ralph entered the theatre to give his first lecture in Auckland was myself, a callow youth just turned 17. He was a stimulating introductory teacher, with the knack of popularising the subject without appearing to simplify or romanticise. His lectures about social structure were illustrated with colour slides of the Kyaka and peppered with anecdotes demonstrating the rigours and realities of fieldwork. His stories also brought to life the leading figures in the history of British anthropology; in that course we learned a little about the ideas and work habits of Frazer, Malinowski, Radcliffe-Brown, Firth, Fortes and Evans-Pritchard as well as about the notables of New Guinea ethnography, from Mikloukho-Maklai, Haddon and Seligman to F.E. Williams, Mead and Fortune and the moderns. He gave out mimeos of well organised lecture notes and recommended we read Colin Simpson's popular but well-informed and highly readable 'travel' books about the opening up of the Highlands. In later years I was to find that his formidable but lightly-worn erudition, spanning all the branches of anthropology and several in biology, made him a fine teacher at all levels.

In 1958 the Anthropology Department was small and closeknit, though its courses covered the full range of disciplines in anthropology. The quartet of Ralph Piddington and Ralph (social anthropology), Jack Golson (archaeology) and Bruce Biggs (Maori studies and linguistics) were the only full-time lecturers. They were supported by a technician, Wal Ambrose, a secretary, Kaye Ponting, and two or three part-time lecturers. A third social anthropologist, Murray Groves, arrived in 1959. Two years later, after Jack Golson had moved to Canberra, two more archaeologists, Roger Green and Wilfred Shawcross, joined the Department. Student numbers swelled and in the mid-'60s there were other additions and replacements among the staff: Jeremy Beckett, Hugh Kawharu and Tony Hooper in social anthropology, David Walsh and myself in linguistics, Pat Hohepa, Sidney Mead and Roger Oppenheim in Maori Studies and Les Groube and Peter Bellwood in archaeology.

These were heady years in Pacific anthropology and linguistics, when Auckland staff and graduate students were breaking new ground on several fronts. The atmosphere for students was marvellous. Undergraduates who showed any interest or ability were drawn into research projects and were soon on first name terms with



The Large Small Game Hunter, with Friends – Kyaka, 1955 (Ellaine Mabbutt)

their lecturers. During the holidays some of the staff and many of the students went together to country places on archaeological digs and Maori Club tours. Almost every weekend there was a lively party at someone's house, attended by Anthropology staff and students and a wide cross-section of Auckland's ethnic and social groups. Ralph was in the thick of all this.

At that time New Zealand was probably the most egalitarian society in the western world. To his colonial colleagues and students Ralph embodied some of the more attractive characteristics of that exotic type (otherwise not widely admired in the Antipodes), the middle class Englishman: he had urbanity and a range of harmless, even endearing eccentricities of hobby and dress: such as his bird-watching, his shirt hanging out (no shirt was long enough to stay tucked in) and his baggy trousers, always threatening to slip to half-mast. But without making any concession in accent and manner, Ralph fitted easily into the New Zealand scene. The secret was that he liked company (though he could be reticent until the ice was broken) and was interested in everything and ready to talk to everyone.

His writings between 1957 and 1965 ranged over double descent systems, bird droppings and plant propagation, elections in the Highlands, prehistoric stone mortars, Kyaka leadership and exchange systems, Kyaka knowledge of birds, Kalam knowledge of frogs, Chimbu plume traders and nature conservation in New Guinea and Kalam social structure. This might seem like the product of a butterfly mind but the interconnectedness of these diverse interests was clearly shown in Ralph's most influential publication from that period: a joint paper with Susan Bulmer called 'The Prehistory of the Australian New Guinea Highlands'. As Golson (this volume, 471) points out, their synthesis rested not only on archaeological data but was "illuminated by a perceptive assessment of the historical implications of the biology, material culture and ecology of Highlands societies". Nearly 30 years later the Bulmers' three-phase sequence for the Highlands remains a point of departure for contemporary prehistorians (as several papers in this volume attest). Another facet of Ralph's activities in this period was his editorial work. He was reviews editor of the *Journal of the Polynesian Society* from 1961-65 and editor from 1966-67. David Walsh once told me that Ralph was the only editor he had worked with who combined *all* the qualities – organisational, critical and constructive – needed in a good editor.

It was not Ralph's fault that I did not become a social anthropologist. I was equally attracted to archaeology and physical anthropology, where Jack Golson mesmerised me with his accounts of the palaeolithic and the rise of civilisation in the Middle East, but I showed no marked talent at these disciplines. Ironically, in the end it was the combination of Bruce Biggs' dry, clinically analytic lectures, and the crowds of attractive Maori and Samoans in Auckland, which drew me into linguistics.

One of the graduate students at Auckland in 1958 was Susan Hirsh, a young American sociologist who had decided to convert to archaeology. She and Ralph soon became good friends and they married in 1959. In September that year they went to Papua New Guinea, where Sue in the Baiyer and Wahgi Valleys and in Chimbu carried out the first archaeological excavations in Papua New Guinea while Ralph observed a series of *moka* exchange festivals among the Kyaka. Bruce Biggs joined them in December and soon after he and Ralph paid a first visit to the Kalam.

VII. KALAM TIMES

Ralph was led to the Kalam by reading a report of a first government patrol in the mid-1950s through the Schrader Ranges which gave an exceptionally detailed account of the local environment and human ecology. The people of this area, on the northern fringes of the central Highlands, had hardly been affected by contact with the outside world; even in the 1990s, it remains relatively remote, with no links by road to main centres. In the Kaironk, Asai and Simbai Valleys lives a sizeable population of horticulturalists who grow the usual Highland crops and keep pigs but also exploit extensive areas of primary forest, rich in wildlife. They have access to a range of forest environments from true upper montane through mid-altitude zones to the Ramu and Jimi lowlands.

Among the Kalam Ralph found the perfect situation to marry his passion for natural history and fieldwork with his training in social anthropology and deep interest in the layers of meaning to be found in human conduct and symbolic systems. I don't know exactly when Ralph decided to make ethnobiology his main line of research or whether he needed a formal charter to combine hobby with profession. The example of Hal Conklin, who pioneered ethnobotanical work in the '50s, may have one of the initial spurs. Lévi-Strauss's work on classification, myth and the cultural significance of animals and plants may have been the clincher.

The Kalam project was associated with a number of innovations in ethnographic method. One was organisational. While it was by no means the first interdisciplinary project in the history of anthropology, it was perhaps the first to incorporate specialists from several branches of zoology as well as other natural sciences into what was fundamentally ethnographic research. Besides the naturalists, whose field sojourns were relatively short, two other social anthropologists (one female, one male), two linguists and an archaeologist undertook long term research in the Kaironk Valley at various times from the 1960s on.

In January 1960 Bruce Biggs and Ralph went to the upper Kaironk. Getting there was not easy. Bruce recalls that Ralph's first plan was to walk from Baiyer River, crossing the formidable Jimi River on rafts. The administration scotched that plan because of reports of fighting among the Kobon people of the middle Kaironk, whose territory they would have to walk through. Eventually, after nearly three weeks of abortive attempts to fly from the Baiyer to Simbai, where the strip was continually closed because of bad weather, Bruce and Ralph flew to Aiome, near the Ramu River, and a few days later made it to Simbai. From there, Bruce writes, "We made our first sally into the Kaironk, taking a very indifferent route through a deep swamp (because, we believed, the carriers wanted to avoid passing by their enemies). Everything Ralph stood on collapsed, of course, fences, bridges, rotten logs. Poor Jomberant would throw himself under Ralph in vain attempts to break his fall, the patrol officer having impressed on him that Ralph's safety was paramount." (Jomberant, a Kalam from the Asai Valley, was their interpreter – at that time no one from the Kaironk Valley spoke Pidgin.)

On that first visit Ralph could stay only six weeks in the Kaironk, having to return to his teaching duties at Auckland, but during that short period he obtained a huge amount of material. Bruce told me how impressed he was with the speed and intensity of Ralph's work.

In 1963 Ralph asked me if I would join him on his second trip to the Kalam, of six months' duration. My job would be to write a grammar of the language for my PhD and to collaborate with him and Bruce Biggs (whose other commitments had drawn him away from the Kalam project) in compiling a dictionary. I had nearly finished writing an MA thesis on Samoan at the time and jumped at the chance to have a crack at a virtually unknown Highlands language.

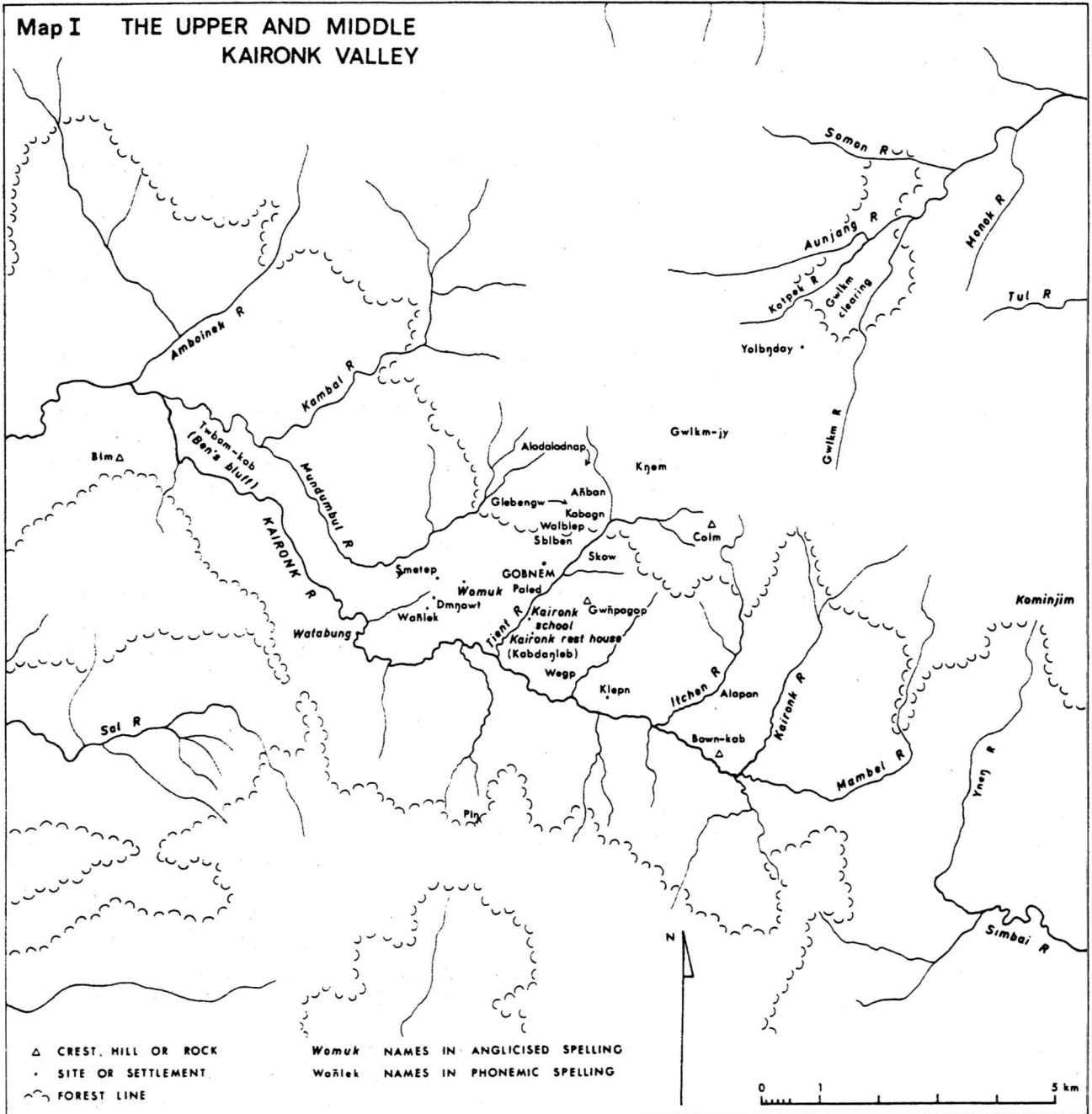
Ralph and I went to New Guinea early in August. These were almost the last days of Empire but as one walked around the coastal towns there was little sense that the colonial era was ending. The hotel bars and dining rooms were exclusively filled by expatriates, mostly rough-talking characters who were served by bare-footed, frizzy-haired men in laplaps. The famous South Pacific company stores, Burns Philp and Morris Hedstrom, were in every town; the expatriates shopped in these while small Chinese family stores catered to an indigenous clientele short on cash. On the plantations the 'native' wage-rate was 30 shillings a month plus keep. The main categories of expatriates were few: Government officers, missionaries, planters and merchants. One soon learned the hierarchy of field officers from District Commissioner down, and the compulsory short name for each rank: D.C., D.O., A.D.O., etc. Expatriates classified indigenes by their district name, simply as 'Sepiks', 'Madangs', 'Chimbus', etc., without the *-an* suffix (comparable to distinguishing different groups of Americans as 'New Yorks', 'Wisconsins' and 'Ohios').

In the pretty little north coast port of Madang I accompanied Ralph to the D.C.'s office and around the stores. He seemed every inch the old hand, completely competent in every detail of planning an expedition. I was surprised by the quantities of trade goods he bought, including axe-heads, bush-knives, leather belts, cloth, blankets, matches, razor-blades, salt, beads, tinned fish and corned beef. At the airport the pilot of the small Cessna weighed us carefully to ensure the load did not exceed the limit of 900 pounds (415 kg). We crossed the northern lowland jungle and flew between the mountain ranges to the head of the Simbai Valley. There the Simbai Patrol Post stands almost at the junction of the Bismarck and Schrader Ranges, the watershed between the Sepik and Ramu catchments.

Our first move, after reporting to the Patrol Officer and overnighing at the Anglican Mission with Peter and Olive Robin, was a two-day walk east to Tsembaga, to see Skip and Annie Rappaport, from Columbia University, who were working among the Maring people (neighbours of the Kalam but very different). En route we camped with Jim MacKinnon, a colourful character who had found gold in the Simbai Valley and had set up his base at Kumburf. We were not yet mountain-fit and it was a hard slog. On arrival at Tsembaga Ralph weighed himself: he'd lost 17 pounds on the walk. He put it all back on again in two or three days, drinking about 10 cups of tea an hour as Skip and he talked on and on about Maring economics.

Then to our fieldwork site. Climbing over the Simbai pass, where the Schrader and Bismarck Ranges meet, one looks down into the Upper Kaironk. From the pass the ranges fork again and for several miles they run parallel, a mile or two apart, framing one of the most beautiful valleys in New Guinea.⁴ Below the forest-clad mountain tops, which rise in places to nearly 3000m, the slopes are covered by a patchwork of grassland, planted casuarina groves and large sweet potato and taro gardens. The main valley is broken up into many smaller hills and vales. Here and there, perched on a spur or a bench of flat ground, is a hamlet consisting of a long 'turtle-back' house and one or two smaller dwellings, with bananas and sugar cane planted nearby. The Kaironk River rises on the north slope, and rushes along with a roar, dropping over 1500m in 20 miles or so until it reaches the flatlands before its confluence with the Jimi; the latter in turn joins with the Lai to become the Yuat, which eventually flows into the Sepik.

The Kalam are strongly built, small people, the men at that time averaging just over five feet (1.53m) in height.⁵ They occupy most of the Upper Kaironk, as well as the Upper Simbai and Asai Valleys. Lower down, the Kaironk is peopled by the Kobon, whose language and culture is quite similar to the Kalam. Our



— from *Birds of My Kalam Country*

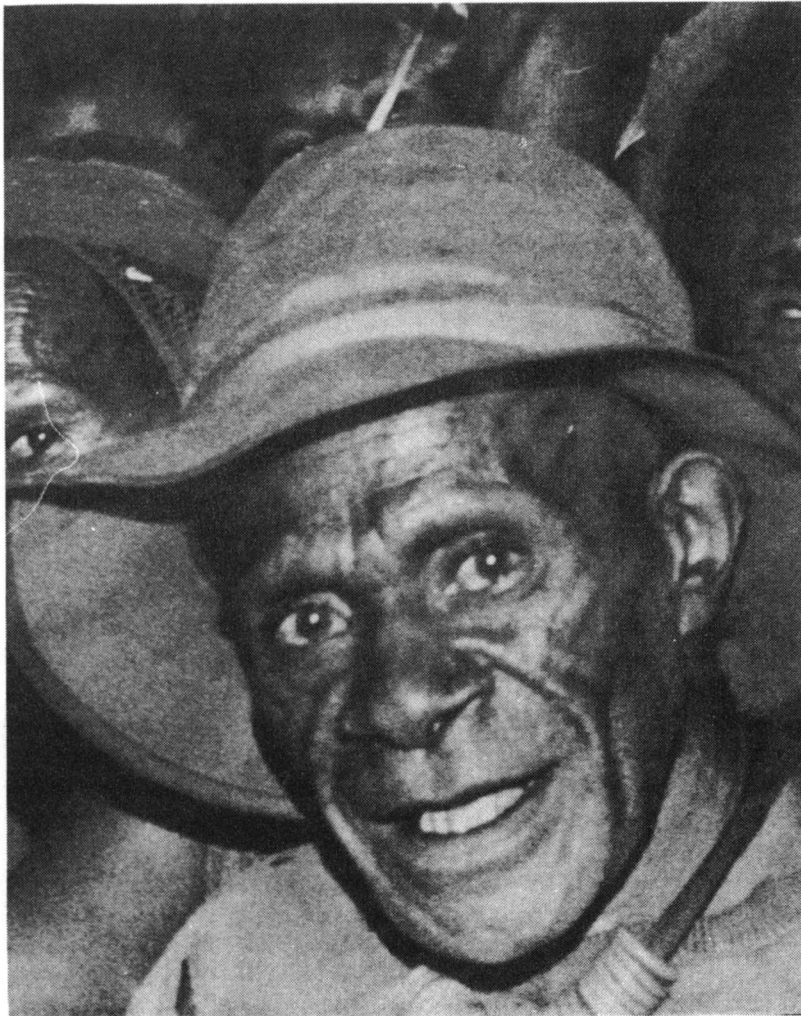
base was at Kabdagleb [kampndangilemp] ridge, at an altitude of 1800m on the north side of the valley, in the territory of the local group known as Kaytog [kaironk].⁶ It was the season for *sm̩y* or dance festivals, where important ceremonies and transactions occur, and during the next few weeks we attended several *sm̩y* in Kalam and Kobon territory. After four or five weeks Ralph moved to Gobnem [gombinim], about half an hour away, where he'd had a small house built for himself and his field assistants. His new base was about 250m higher up the valley wall, a bit closer to the forest and right next to the homestead of his friend Wpc [úpič] (or Oovich, in popular English spelling), the Big-man of Gobnem.

Ralph and Wpc had hit it off in 1960 and later they became the greatest of mates. Years later Ralph wrote warmly of Wpc's hospitality and "the wit and exuberant good humour which sustained a truly marvellous quality of life in the little community he dominated" (1977:15). The Kalam called Ralph *Byob* [mbiyómp] 'Big-man' — a term doubly appropriate⁷ for, despite his complaints about who on this or that day had been pestering him for things he revelled in the big-man role. He took on a largish staff of full- and part-time field assistants and entered into exchange relationships with Wpc and his family and others in the Gobnem

community. These 'exchange' relationships continued for the rest of his days. He regularly sent money and other gifts from Auckland and Port Moresby to his closest friends and helpers.

His fieldwork practices were, in nearly every respect, a perfect model for any young player. He took notes in several small pads, one for each main category of information. One of his notebooks served as a diary, another as his register of specimens, and so on. All were written in a miniscule handwriting that no one but he could decipher. As soon as he took a photograph – and he took a great many, of every person, place and ceremony, using a heavy Rolleiflex camera and a light-meter – he jotted down the full details. He made tape recordings of all sorts of speech events and took the occasional bit of cine film. And of course, he collected specimens endlessly, or rather his army of collectors did. I was dragooned into being a specimen-receiver as well as assistant ethnographer and photographer. People brought in rats, birds, spiders and so forth and I was supposed to record the name, date and collector of each on a label before slipping it into a box or a drum of spirits. These creatures were paid for on a sliding scale and one had to remember that a frog was worth more than a beetle and that one kind of bush rat, being harder to capture, was worth more than another. One day Ralph scolded me severely when he found I'd put a rat into the drum without first gutting it. He was known to explode when provoked and from time to time he roared at assistants who had really annoyed him, as one day when he found that they hadn't bothered to tell him, until too late, about a certain festival he'd wished to attend.

Almost every day he walked up into the forest or around the grasslands and gardens, carrying his binoculars, with a retinue of half a dozen men and boys who toted his camera, gun and the specimens they had collected. From Kabdagleb in the late afternoon we were likely to see the figure of Ralph emerge from forest or gardens far above us, lumbering above a group of small companions, and slowly make his way down a ridge. If it was a ridge leading towards our camp, the cry would go up '*Byob asaw!*' ('Big man is coming!') and I would put the kettle on and heat a drum of water for his shower. In the evenings we drank 'Negrita' rum with passionfruit juice and conversed over dinner, usually tinned meat and local vegetables, in our hut.



Ralph's friend Wpc (Oovich)

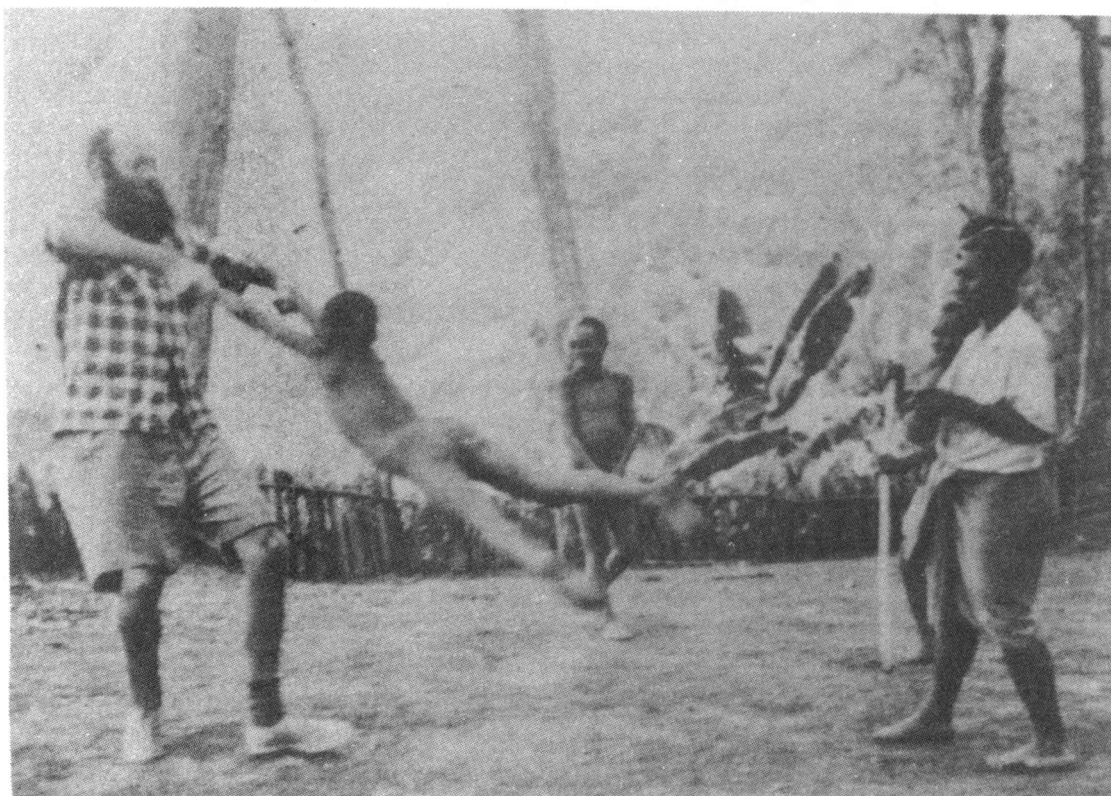
On Sundays Ralph took it easy. His ritual was to lie in his canvas stretcher reading one issue of *The Observer* from cover to cover, either an ancient issue which he had lined our trunks with or one that had arrived (three months after publication) in the mails that were fetched from Simbai once a week.

Ralph was that rare thing, a British-trained social anthropologist interested in structural linguistics as opposed to semantics. He understood the phoneme and the morpheme and took an encouraging interest in the progress of my linguistic researches and in the tangible products (from portable typewriter and tape-recorder) that I sometimes showed him. Though he spoke tolerable Kyaka, he never became a fluent speaker of Kalam, a difficult language, on which I have written elsewhere in this book. He spoke mainly Pidgin to his young assistants with frequent interpolations of English-accented Kalam. But he gained a truly profound analytic knowledge of Kalam lexicon and semantics, and the translations of Saem Majnep's Kalam texts, which he did in the 1970s and '80s, are masterly. I learned a great deal from him about making dictionary entries. Our dictionary was very much of the 'encyclopaedic' type. Like most social anthropologists, however, he could not resist proposing fanciful etymologies for many words and he accepted that one of my roles was that of etymology-squelcher.

One night, after about three months in the field, we held a celebration. The 1963 expedition had been partly floated on expectation of a grant from an American foundation and Ralph was mightily relieved when the post brought a letter announcing he'd been awarded \$US 3500. It was a sizeable sum for those days and, I believe, the largest grant Ralph ever had. That night, as the levels in our whisky and rum bottles fell, his cloak of reserve dropped away and he told me about his early life and loves.

In spite of the remoteness of the site from the outside world there was a steady traffic of natural scientists through our field camps, each staying a week or two and walking about collecting with Ralph. One of the visitors was a frog man (Mike Tyler), another a rock man (John Chappell), and so on. Ralph knew well the advantages of working with specialists. Jared Diamond (this volume, 85) tells Ralph's nice story about how, after steadfastly maintaining to him that they had only one name for all rocks, Kalam informants immediately gave a geologist visitor, John Chappell, lots of names for different kinds. To an indignant Ralph they explained that after walking about with the geologist it was obvious to them that he knew a lot about rocks whereas Ralph's questions showed that he knew nothing about this subject!

I must tell you about my small part in Ralph's celebrated cassowary paper. We had for weeks been trying without luck to elicit folktales and myths from the Kalam. Our informants flatly denied they had anything like the European, Polynesian or Kyaka tales we offered as examples. Then one day a small boy, I forget why, told me a myth-like story which he called a *sosm*. Suddenly the floodgates opened and within two days I had recorded 30 or 40 *sosm*. Ralph then had similar success, paying a box of matches or a razor-blade for each



Ethnographer Swinging! (Christine Helliwell, and *Canberra Anthropology*)



Gwüpagop Knoll and Colm Mountain from Matpay, Kaironk Valley (Ralph Bulmer)

story; *sosm*-tellers came from far and wide and soon more than 100 stories were on tape. Among a small sample of transcripts and translations of tales I had sent to Ralph at his house up the hill at Gobnem was one about a woman who, after a series of adventures with her brother, fell into a pit-trap and changed into a cassowary. When Ralph called by a couple of days later he was very excited by this *sosm* about the cassowary woman, saying it allowed him to fit together various bits and pieces of Kalam world view that he'd been puzzling over. His ruminations about oppositions between nature and culture, wild and domesticated, forest and garden, affines and agnates, etc. were largely wasted on a linguist more concerned with unraveling the morphology and syntax of Kalam. At that time, prosaic and narrow of mind, I found it hard to see them, no matter how carefully argued, as testable hypotheses. But I must have been a useful sounding board because after his next field trip he paid me a backhanded compliment, saying, "I didn't generate so many ideas this time, not having you around to try them on."

Ralph took his responsibilities seriously. Towards the end of that spell of fieldwork he felt obliged to report the Patrol Officer in charge of the Simbai Post to his superior, for allowing his police to 'soften up' several Kobon prisoners suspected of having killed four people in a vendetta raid. It cannot have been an easy decision, because we had enjoyed excellent relations with the P.O., and anthropologists in the field were dependent in various ways on the goodwill of the local field officers. A mutual friend remarked to me afterwards, "I think I would have kept quiet about it." Though the P.O. got into a certain amount of hot water over this incident, he readily forgave Ralph. Ralph's integrity came out in many small ways. He was, for example, scrupulous about providing reports on his fieldwork and copies of useful documents, photos, etc. to the appropriate people and institutions of government and education in the places where he worked.

The project continued and expanded through the 1960s. In 1965 two young Kalam men, Simon Peter Gi and John Kiyas, spent six months in Auckland. Around the same time two social anthropologists, Graham Jackson from Auckland and Inge Riebe from Sydney, began working in the Kaironk Valley among the Kobon and Kalam, respectively. A few years later archaeological excavations were carried out by Sue Bulmer and Ian Saem Majnep. Ralph had met Saem in 1963, when he was impressed with the 15 year old lad's knowledge of plants and animals and his all-round savvy. Within a few years Saem had become his chief field assistant and Sue's as well.

Back in New Zealand the papers on ethnozoology started to pour out. Some of the early papers, with austere titles like "Kalam Classification of Frogs" and "Kalam Classification of Marsupials and Rodents", Ralph spoke of as being more or less 'straight' ethnography; their main purpose was to document Kalam zoological knowledge and taxonomies.⁸ But other papers were theoretical, seeking to lay bare the cultural and intellectual bases of taxonomic systems and anomalous categories. His demonstration that the Kalam taxonomy of wild animals is a rational system of a very complex kind, grounded partly in natural kinds⁹ and universal principles of cognition but also partly in everyday social and economic concerns to do with marriage, land inheritance, warfare and so on, was widely recognised as a breakthrough made possible by combining theoretical speculation with meticulous ethnography.¹⁰

To some of the theoretical papers he gave memorable titles, often with a touch of Lewis Carroll in them: "Which Came First, the Chicken or the Egghead?", "Trees, Grerbs, Wugs, Snurms and Quammals", "Worms that Croak and Other Mysteries of Kalam Natural History" and "Why Is the Cassowary Not a Bird?" He introduced a few new words into the technical vocabulary of ethnobiology. Two which have taken root are *game mammal*, those wild marsupials and large rodents prized as game by New Guinea hunters (Kalam *kmn*), and *specieme*, a well-defined natural kind, perceived as such by members of a society. Another was *undercroft*, which Ralph used not in its usual sense of the vaults below a cathedral, but to translate the Kalam term *abn*, which denotes the cavities and galleries underneath the floor of the tropical rain forests, in which countless animals live. He pointed out that this is quite a different thing from what ecologists call the forest litter-layer.

Ralph went to Europe for his sabbatical in 1966. He and his family spent most of their time at Cambridge but he was already forging links with French anthropologists which he maintained for the next two decades. He visited the ethnobotanist Barrau and told stories of calling on Lévi-Strauss at his flat.

At the end of 1967 Ralph left Auckland to become the first Professor of Anthropology and Sociology at the University of Papua New Guinea. The Bulmers were farewelled with a memorable party at the house of Tony and Robin Hooper.

VIII. THE UNIVERSITY OF PAPUA NEW GUINEA 1968-72

The late '60s and early '70s was an exciting period in the history of what was to become the nation of Papua New Guinea. Independence was just around the corner and everyone knew the country was ill prepared. Some very able people, attracted by the challenge of making a contribution, came from various countries to head the major departments of the newly-established University, which opened its doors to students in 1967. Helga Griffin has written a fine nostalgic piece recapturing the ambience of the UPNG in that period.

Ralph's Department comprised a veritable zoo of disciplines: comparative religion, social work and community development, linguistics and prehistory as well social anthropology and sociology. I came for a year in 1969 at Ralph's invitation to establish Linguistics courses.

Hordes of social and natural scientists (and not a few Kyaka and Kalam visitors) descended on the Bulmers' house in Waigani and received shelter, food, good company and wise words of advice. Michel Panoff's word portrait at the head of this essay captures an experience which happened to many a visitor. It would hardly be an exaggeration to say that every anthropologist and naturalist who came to Papua New Guinea in those years is indebted to the Bulmers. Sue was a tower of strength. Although at the time she herself was carrying out excavations in the Moresby area for her PhD and raising three lively youngsters, most weeks she took care of several visitors who camped in the house; and the Bulmers never failed to invite other colleagues home to dine and talk with each set of visiting colleagues.

These were vintage years for Ralph and he drank deeply of them. Years later he spoke of his sojourn at the UPNG as one of the happiest periods of his life. Although his research suffered because of his teaching and administrative load, he was otherwise in his element, establishing institutional structures for teaching and research in anthropology and to protect and foster the economic, social and political interests of traditional communities in a rapidly-changing world. He founded a new journal, *Man in New Guinea*, based in the Department. But his impact in the community extended far beyond his own Department and subject.

IX. AUCKLAND AGAIN

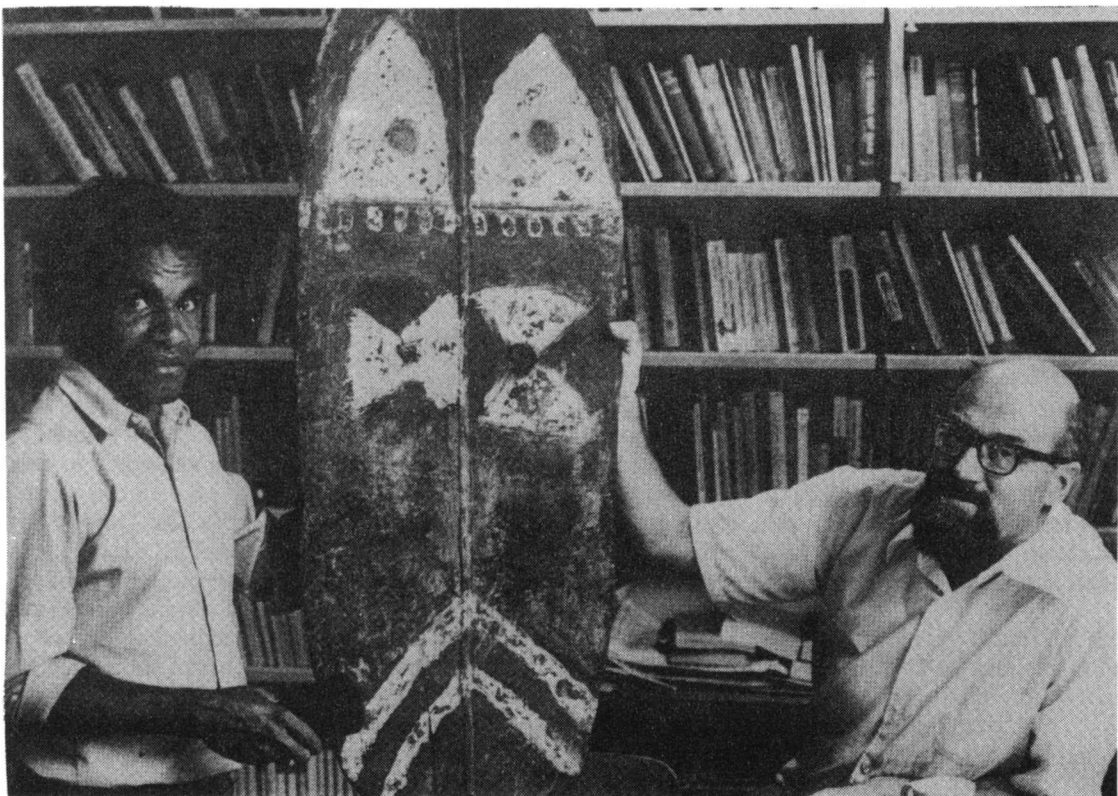
With their children approaching high school age, Ralph and Sue felt it would soon be time to leave PNG. The vacant chair of social anthropology was offered to Ralph and they returned to Auckland in 1974. First, however, they took a sabbatical. Ralph was a visiting fellow at Clare Hall, Cambridge. In June 1973 he attended an ethnosience conference at the Maison des Sciences de l'Homme in Paris. His own contribution to the conference (Bulmer 1974) begins with a Bulmeresque touch: a taxonomy distinguishing five basic types of ethnobiologist, into which he fits the French scholars (among others) whose work he is familiar with,

including Barrau, Dournes, Friedberg, Godelier and Martin. The redoubtable polymath Haudricourt he declares unclassifiable.

Ralph found the University of Auckland and New Zealand society much changed. For one thing, the University was a much bigger and more impersonal institution. Anthropology and Maori Studies had become all the rage: in 1975 over 1000 students enrolled in the first year social anthropology course and as no room was big enough to accommodate them all each lecture had to be repeated. The Department of Anthropology, with a teaching and technical staff now numbering almost 40, and with scores of graduate students, was now more like a School housing five departments with divergent interests than the close-knit little department it had been. Ralph's research took third place to administration and teaching. One of the courses he taught regularly, on folk classification, was known to the students as 'ethnofrogs'. Another, on the applications of anthropology, was hugely popular with middle-aged ladies and Ralph received some good-natured chaffing on this score. He had considerable *mana* in the University but he did not capitalise on this as much as he could have, seemingly unaware of his own prestige. Though humble about his own accomplishments, Ralph had, when I first knew him, given off an aura of competence and self-assurance. One felt that he always knew what should be done in any given situation. This was less so as the 1970s passed. There were more moments of palpable self-doubt.

After 20 years cracks had appeared in Ralph and Sue's marriage as two busy people tried to pursue separate and demanding careers. They both took their eventual separation hard and he left for his 1980 leave an unhappy man. He spent the Spring Semester of his leave as a Distinguished Visiting Professor at Rutgers in New Jersey (we smiled at the humour of an ethnozoologist visiting a Fox and a Tiger) and gave lectures at several other universities in the US. His leave report emphasises how much he enjoyed talking with his US counterparts in ethnobiology. A spell of fieldwork in the Kaironk followed, then in Paris, between November 1980 and January 1981, he gave a series of five or six seminars which are still talked about there. In these he talked about the collaborative projects with Saem and the complex methodological and ethical problems which they presented.

Here was perhaps the most remarkable innovation of the Kalam project: the writing partnership between an anthropologist and a Kalam naturalist and hunter. Both George Marcus and Saem Majnep have written elsewhere in this book about the development of this partnership and I will give only a few details here. In one of his Paris seminars Ralph describes how the idea of writing books with Saem came as a "spur of the moment decision, or inspiration" late in 1974. It is true that Ralph surprised Saem on the latter's arrival in New Zealand in December that year with the proposal that they write a joint work about birds. And it is true



Ian Saem Majnep and Ralph Bulmer at the University of Auckland at the Time of Publication of *Birds of My Kalam Country* (Auckland Star photograph)

that they held the interviews and put together the text of *Birds of My Kalam Country* in just two months that Auckland summer (of course, after years of groundwork). However, the germ of this proposal must have been in Ralph's mind for some time. On page 24 of his inaugural lecture at the UPNG (Bulmer 1969), you can find Ralph, stimulated by the Panoffs' (1968) book on the methods of social anthropology, already looking for alternatives to the conventional ethnographic report in which the professional investigator presents his 'external' perspective. The lecture mentions research contexts in which members of the community under study might appropriately serve as joint authors or even as the primary investigators and authors.

Saem's next visit to Auckland in 1977 coincided with the publication of the *Birds* book; it was celebrated with a memorable party. By that time a second and even more ambitious book was under way: *Animals the Ancestors Hunted: an Account of the Wild Mammals of the Kalam Area, Papua New Guinea*. The *Animals* book was a very different kind of project from *Birds of My Kalam Country* (fuller accounts of how the two books were conceived and created are given elsewhere in this volume by George Marcus and by Saem himself). Saem had recently learnt to write Kalam and now he not only planned the structure of the book but dictated each chapter onto tape in Kalam and then transcribed the text. By the time he left, three months later, Saem had transcribed first drafts of about 20 chapters. However, the task of retranscribing, editing, translating, polishing translations and adding supplementary chapters, commentaries, footnotes, illustrations and glossaries was to take much longer.

XI. THE LAST YEARS

The toing and froing of Ralph to New Guinea and Saem to New Zealand continued. When Saem visited Ralph in Auckland at the end of 1982 they revised the Kalam text and the translations of the *Animals* book and wrote a paper on food plants of the forest. The following year Ralph married Lena Lane, a nurse whom he met when she studied anthropology at Auckland. Their son Richard was born in 1984. Ralph's last field trip to the Kalam was in 1985, when he and Saem began their ethnobotanical project in earnest with a study of the genus *Ficus*. It was hard for Ralph to find the time needed to finish *Animals*. Between 1984 and 1987, for



Ralph in the 1980s (Marlous Terwiel)

instance, hundreds of hours of his time were taken up by a few disputatious colleagues. His plan had been to publish an English-only edition first (mainly because this was what commercial publishers would require) then to find a publisher for bilingual and perhaps Kalam-only editions. In 1987 he said "I feel very bad, especially for Saem, about taking so long to get the book out. I've only just realised that I've been doing things the wrong way round. The job would have been a lot easier if we'd put out the bilingual version first, as a series of working papers, and then used these as the basis for a polished English-only edition." He was going flat out on the working papers and had pretty well finished editing the first three of the planned series of 12 when lung cancer, manifesting itself first as a brain tumour, hit him in January 1988.

A few years earlier, Ralph had asked me if I'd be his literary executor. Neither of us imagined there would be any need to talk about such things again for a long time. Now we made contingency plans for finishing the *Animals* book and other projects. He continued working on the book as long as he was able. He remained determined to recover and wanted to delay Saem's coming until next year when, he declared, he would be freer to concentrate on the plants work. His family rallied around him. Lena's nursing skills were invaluable during these trying months and she was a hospitable hostess to Ralph's many visitors. There were many things one wanted to ask, many things that needed to be done in connection with this or that matter but it was not possible to badger him. He reserved a good part of his time for his children and for writing to friends. He tape-recorded for Richard a children's story, *Kwibep*, based on the habits and odd appearance of the Highlands water-rat, which he had written during his last field trip.

Late in June Saem arrived, carrying with him 13 chapters of *Kalam Plant Lore*. Saem's quiet presence was good for Ralph and they talked a little each day. In early July Ralph learned from John Lynch, Vice-Chancellor of the University of Papua New Guinea, that the UPNG intended to award Saem an honorary DSc. for his work on Kalam ethnobiology. The degree was conferred in 1989. Near the end we showed Ralph the list of contributors and titles for the *Festschrift* and he shook his head and said, "I don't deserve it. There are so many *Festschriften* for other people that I didn't get around to contributing to." But next day, anxious that no deserving colleague should be slighted he asked to see again the full list of invited contributors and suggested one or two additions.

There are many of us who miss not only Ralph's vast general knowledge and critical acumen but also his sense of the ridiculous, his mad scientist's chortle, and his pleasure at hearing of a new idea. Frequently I find myself wanting to share with him a little discovery or observation – he was usually delighted by such revelations and gave good comments because he could integrate almost any bit of information into his world. After a split second's pleasure anticipating his response comes disappointment as one realises: "Damn! He's not there to tell any more." It is hard to believe that such a large personality and so much knowledge and wisdom are gone.

Thinking of Ralph's long Kalam project, as I often do, I am reminded of Dr Johnson's reply to a woman who at a social function proffered sympathy to him on the headaches and drudgery Johnson must have put up with during the ten years he spent making his dictionary. "It must have been terribly hard for you." "On the contrary, Madam," said Johnson, "It was nothing of the sort. I knew what to do, I knew how to do it, and I did it well."

NOTES

1. Luckily Ralph himself put on paper a good deal of autobiographical material. The richest sources on his professional life are a paper, "Memoirs of a Small Game Hunter . . ." (Bulmer 1974), which describes in some detail his field methods and his thoughts about issues in ethnobiology; a body of unpublished notes labelled 'Paris papers': handwritten and typed notes for half a dozen seminars he gave in Paris between November 1980 and January 1981; and various unpublished reports on his periods of leave and fieldwork. Also he was a great writer of letters, though I have so far not been through much of his professional correspondence. I am grateful to many people who have provided information for and/or critically read this piece, especially Rosemary and Ross Pownall, Alice Bulmer, David Bulmer, Kenneth Bulmer, Susan Bulmer, Lena Bulmer, Bruce Biggs, Lois Carrington, Derek Freeman, Jack Golson, Roger Green, Robin Hide, Graham Jackson, Aletta Biersack, Harold Brookfield, Inge Riebe, Marie Lawrence, Ellaine Mabbutt, Skip Rappaport, Ian Whitaker, Marie Reay, Hans-Peter Stoffel, Darrell Tryon, and David Walsh.
2. My own acquaintance with Ralph began in 1958, when I took a course in anthropology from him, the first of several over the next few years. We did fieldwork together among the Kalam in 1963-64 and later collaborated on a dictionary of Kalam. During the years 1965-67 and 1976-88 I was one of his colleagues in the Anthropology Department at Auckland and in 1969 at the University of Papua New Guinea.
3. In fact, Ralph's first wish was to go to Africa to work among the Bushmen, but the Australian National University offered doctoral scholarships and ample research funds.
4. Ralph often commented on the similarity in appearance of the Kaironk Valley to his beloved North Wales.
5. The Kalam have increased appreciably in stature over the past 30 years.

6. Kalam terms are generally cited here in phonemic orthography, sometimes with a phonetic transcription added. Readers sometimes complain that the Kalam orthography is an abomination, impossible to read, and should be replaced by a phonetic spelling. My reply is that the phonemic orthography, by definition, represents the structure of the sound system, as Kalam perceive it, far more systematically than a phonetic transcription. Furthermore, the main rules for pronouncing the Kalam orthography can be learnt in a few minutes from a key. See Pawley (this volume, p.432ff) for a brief account of Kalam phonology.
7. Despite Bruce Biggs' claims, I never actually heard the Kalam refer to Ralph as 'man and a half'. However, Stephen Wurm says that on a visit to Tabibuga he heard talk in Pidgin about '*wanpela bikpela man tru, olsem tupela man, i kam long Simbai*' ('a huge man, as big as two men, has come to Simbai'). En route to the Baiyer River in 1954, Ralph and Ellaine visited Marie Reay in the field, and Marie recalls the reactions of the Kuma: "[Ralph's] arrival drew a vast crowd of people to gawk at the biggest man they had ever seen. Several people ran off to fetch relatives . . . Children, pushed forward to have a look, were frightened until Ralph won them over. Konts, an exceptionally tall man, sidled up to Ralph, measuring himself against him and exclaimed, wonderingly, *Yi kembis mei* 'I am only a little man'.
8. Ralph described his ethnobiological writings as dividing into two main classes: works that are largely data reports (his articles co-authored with colleagues on Kalam classification of frogs, reptiles, birds, mammals, etc.) and papers that draw theoretical implications from these data. But this is an overly modest view of his taxonomy papers. They are not just about Kalam categories. They are about (1) the nature of the cognitive process in categorising and recognising, (2) the nature of nomenclatures and taxonomic systems, and (3) the interaction of (1) and (2).
9. Ralph acknowledged that his thinking on natural kinds owed much to Ernst Mayr:
Of the biologists whose works I have read, the one who has affected my thinking most is Ernst Mayr. This is . . . because his stance on the species concept and his review of the historical relationships between typological and biological species makes such good sense of my own experience as an amateur naturalist . . . This in turn predisposes, or biases, me to make sense of Kalam experience of animals and plants, as I record this, by the use of his concepts. Of course my initial interest in birds . . . influences me here. It could be that if my initial enthusiasm had been for plants or earthworms, I would have been less impressed by *Systematics and the Origin of Species*, and would, more recently, have made greater efforts to find direct relevances of numerical taxonomy to systems of folk-classification. Certainly I must recognise that I am biased towards discovering what Mayr calls 'local species' or 'non-dimensional species' . . . in, or underlying, systems of folk taxonomy. In my reports I attempt to give full weight to evidence that would appear to run contrary to this expectation . . . , but I need the assistance of critics who are prepared to read my data-papers very carefully, to prove me wrong or, at least, to define the limits of utility of my formulation (Bulmer 1974: 89-90).
10. He was however notably cautious about proposing universal categories and explanatory principles. In this connection Terry Hays writes:
To the best of my knowledge, Ralph Bulmer never published a general "explanation" of Kalam folk biological classification systems. I suspect that this is not because of a lack of interest . . . or a failure to appreciate the use to us all of such a culmination to his decades of exploring that particular corner of the human world and mind. Rather, I think it is because the tutelage of Saem Majnep and his own passion for the natural world around him made him ever-cognisant that what often "counts" – but resists counting – is the *salience* of organisms (Hays, this volume, 113).

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PART II

ETHNOBIOLOGY, SEMANTICS AND TAXONOMY



THE CHICKEN AND THE EGG-HEAD REVISITED: FURTHER EVIDENCE FOR THE INTELLECTUALIST BASES OF ETHNOBIOLOGICAL CLASSIFICATION

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... the discontinuities in nature. . . which zoologists and botanists regard as species. . . would always have been regarded as species since zoology and botany became sciences, regardless of the theoretical rationales applied (Ralph Bulmer, 1970:1083).

One of the major issues in ethnobiology¹ concerns the growing debate surrounding the *bases of ethnobiological knowledge*. It is a curious debate, and one that shows something of the cyclical nature of alternative explanations in social science that are popular at one moment in time, then fall into disfavour, only to be rediscovered again by a younger generation of scholars.

The current focus on the bases of ethnobiological knowledge includes proponents who can be readily grouped into two camps. Borrowing some terminology from Terence Hays and Darrell Posey, these two camps can be conveniently named the *intellectualists* and the *utilitarianists*. Proponents of the intellectualist position argue that ethnobiological knowledge, and especially that knowledge associated with the classification of biological diversity, is fundamentally cognitively motivated. There are at least two varieties of the intellectualist stance. The first and historically prior is associated with the French structuralism of Claude Lévi-Strauss and is best articulated in his important work, *The Savage Mind* (1966). A second version was later developed in the United States by ethnobiologists trained in the tradition of ethnoscience, and in New Zealand, as seen in the pioneering work of Ralph Bulmer (Atran 1985a, 1985b, 1987, 1990; Berlin 1972, 1973, 1976, 1992; Berlin, Breedlove, and Raven 1973, 1974; Berlin and Swift, in preparation, Boster, Berlin, and O'Neill 1986, Boster, n.d., Bulmer 1967, 1968, 1970, 1974a; Bulmer and Tyler 1968, Bulmer and Menzies 1972-73, Hays 1974, Hunn 1977, 1979 [but see below], Patton, Berlin, and Berlin 1982). While the two varieties of the intellectualist position differ in important ways (see Berlin and Berlin 1983), they both share the view that human beings everywhere recognise the *inherent order and structure* in the biological world quite independently of whatever practical value plants and animals may be thought to possess.

The utilitarian position, represented in the work of several ecologically focused ethnobiologists in England and France, as well as a number of American ethnoscientists who have abandoned their earlier intellectualist stance, presents a modern version of the functionalist views of the great ethnographer Malinowski who saw ethnobiological knowledge as the quintessence of primitive pragmatism (see Morris 1984). In Malinowski's words, "The road from the wilderness to the savage's belly and consequently to his mind is very short. For him the world is an indiscriminate background against which there stands out the useful, primarily the edible, species of animals and plants" (Malinowski 1974:44, original edition 1925). The neo-Malinowskian utilitarians claim that the major purpose of ethnobiological classification is to assist human populations in adjusting to their particular habitats by giving names to just those plant and animal species that have practical consequences for human adaptation (see Hays 1982 for a critical summary of this position). The recent writings of Hunn 1982, Morris 1984, and Brokenshaw and Riley 1980 are most directly typical of this position. A recent paper by Posey (1984) represents an attempt to arrive at some middle ground between the two views. (Broader, although ultimately neofunctionalist arguments that open a Pandora's box of possible sociological and symbolic explanations in ethnobiological classification can be found in Ellen 1986 and Ellen and Reason 1979, 1986; Friedberg 1979, Sillitoe 1980, 1983; Dougherty 1981, Randall and Hunn 1984, and Hallpike 1979. I will be concerned in this paper with addressing the strongest form of the utilitarian hypothesis as set forth most clearly by Hunn (1982) who, unlike some of the more sociologically oriented approaches, takes care to form his argument in a concise and coherent fashion.)

In what ways might the major differences in interpretation separating the utilitarianists and the intellectualists be fruitfully explored, and what kinds of evidence would be critical in evaluating the plausibility of either argument? A clue may be found by examining the differences between intellectualists and utilitarianists on the issue of *which* plants and animals are singled out for naming in any particular ethnobiological system of classification. Although obvious, it is first necessary to point out that no system of biological classification, including the taxonomy of modern biological systems, is totally comprehensive in its categorisation of the living world. The number of organic beings on the earth runs to the millions and systems of folk botanical and folk zoological classification name but a small proportion of the naturally occurring

species of plants and animals in any particular localised habitat. What, then, underlies the choice of individuals in pre-literate societies to name and classify those biological organisms that *do* get labelled?

For the utilitarianist, the answer seems simple enough. Hunn, perhaps the leading proponent of utilitarian arguments in ethnobiology today, states that since there are far too many distinct pieces of biological reality to be dealt with linguistically, "... the alternative... is to impose a selective process based on *utility*. If we are to explain why a particular subset of the available natural discontinuities is selected for cultural recognition, we must model this selection process. This requires us to consider the *practical consequences* of knowing or not knowing some plant or animal" (Hunn 1982:834, emphasis added). Or, as Posey has stated, "[intellectualists] do not explain *why*. . . certain natural domains are classified and named while others are not. This question is best investigated from the utilitarian/adaptationist approach" (Posey 1984:123, emphasis in original).

There are a number of logical and procedural problems with this *a priori* position. If the plant and animal species named in systems of ethnobiological classification were selected for linguistic recognition primarily on the basis of their utilitarian importance, then it should in theory be impossible to predict which classes or organisms will be named prior to conducting relatively complete ethnographic and ethnobiological investigations in any particular society. One of the major lessons of ethnography is that the belief systems of human societies are multifaceted and culture-specific. The relativist utilitarian would surely be the first to proclaim that the pragmatic, culturally defined significance of plants and animals must be *discovered* not *deduced*. On the other hand, to the extent that one is able to predict which plants and animals in some society will be named *without prior knowledge of the cultural significance* of these organisms, the utilitarian argument loses much of its force.

How might one approach the problem of predicting which of the thousands of organisms in any particular local habitat might have the privilege of being named? The space available to me here is far too short to elaborate the full argument (see Berlin 1992), but its essential outlines can be seen by considering the following, not-too-hypothetical example.

Let us imagine a day in the rainforests of the Amazon in the late 19th century. A European naturalist, someone with the scientific curiosity of a Bates or a Wallace, is actively engaged in systematic zoological collecting. His goal is to develop a local faunal inventory of the major vertebrates found to inhabit the forest and streams of an area transcribed by, say, a 30 kilometre radius of his field site. As he begins his collections, what does he see? He is not confronted by a vast continuum of animal species gently and imperceptibly merging one into the other, species *x* gradually becoming species *y*. What he perceives is an array of discrete, discontinuous pieces of biological reality. As Ernst Mayr stated years ago, "At a given locality, a species of animal is. . . separated from other sympatric species by a complete gap. This is the [unidimensional] species of Ray and Linnaeus" (1969:37). Furthermore, some of these species are seen to be more similar to one another than others and some are so strikingly unique as to be thought of as unrelated to any other species in the area.

On the basis of these "... observed similarities and differences among groups of organisms" (Mayr 1981:510), our naturalist now begins to classify the animals he has collected. He begins to arrange them in groups based on their overall similarity or resemblance (Simpson 1961). In addition, he begins to note that some organisms can be grouped into ever more inclusive groups. This is possible because he recognises, along with Darwin and many pre-Darwinian biologists before him, that "... all organic beings are found to resemble each other in descending degrees, so that they can be classed in groups under groups" (Darwin 1859:431).

In constructing his classification, our natural historian finally attempts to codify the perceptual facts with which he is presented. He will observe that the differences between two groups or organisms are relatively small, and he will want to place them relatively close to one another in his growing taxonomy. Other creatures will appear to be perceptually very different, and he will place them rather far apart in his classification. Being a good taxonomist, he will try to construct a hierarchy of groups under groups which recognises that the differences between two species of the same genus represent a perceptual difference that is considerably smaller than those differences which separate two genera of the same family. Likewise, he will note that two genera of the same family are more similar to one another than either is to a genus in a separate family (see Figure 1).

To continue with our example, imagine now another zoologist residing in the same area where our 19th century natural historian is presently collecting specimens. Unlike our western trained taxonomist, this person is illiterate and has no particular interest in producing a faunal inventory of the vertebrates in the region, although he could easily do so from memory. He has even worked as a collector on an occasional basis with the European who has found his knowledge of the local fauna to be rather extensive. As you will now have guessed, this zoologist is an Indian.

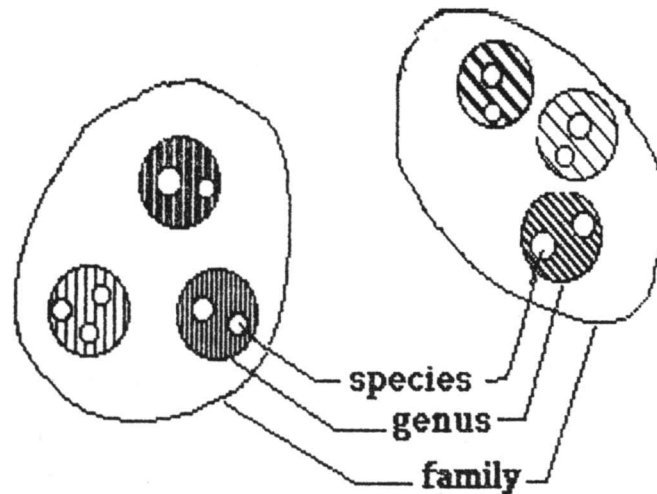


Figure 1. Schematic grouping of taxa by degree of similarity (after Simpson, *Principles of Animal Taxonomy*, 1961:117)

Along with the European natural historian, our indigenous zoologist will have looked out upon the same forest and streams, will have regularly seen most of the same animals, and will have developed an implicit system of ethnozoological classification. And, along with the western scientist, he will have used exactly the same kinds of perceptual cues of similarity and difference in the recognition and consequent naming of groupings of organisms. An obvious but necessary qualification is required here. Notice that in the preceeding passage I wrote "... will have seen most of the same animals". If our Western zoologist had special collecting equipment, such as fine-meshed bird nets, and if he worked systematically at night to collect bats, he will be likely to have captured a number of these rapid flying, nocturnal creatures that our indigenous ethnozoologist will never have seen. This is simply because the behaviour of these animals makes them essentially unobservable by native peoples.

Now we are ready to consider the intellectualist and utilitarianist positions on which groupings of animals are likely to be named. Again, Hunn, speaking for the utilitarian view, is unambiguous: "[Ethnobiological] classification is *highly selective and [knowledge of] the practical significance of organisms is important in the selection process*" (Hunn 1982:835, emphasis added). If this is true, one would have to spend a good deal of time talking with our indigenous ethnozoologist prior to making any predictions on how he might classify the species of the region.

The intellectualist argument, however, leaves issues of utility aside as concerns the underlying bases of ethnobiological classification and makes a straightforward prediction: To the extent that the western scientist and native ethnozoologist see the *same species* [of vertebrates], and to the extent that the two systems form their groupings on relative degrees of similarity and difference among the species, then the western and folk systems of classification should potentially correspond closely as regards which species are singled out for naming. Since they are classifying exactly the same animals, the taxonomy of the European, based on no knowledge whatsoever of the cultural importance of the animals he is collecting, should be nearly identical to that of our indigenous ethnozoologist. If this is true, one should be able to use the scientific system as a road map for predicting which organisms will be named in the folk system.

Before moving on to look at some actual data, let me explore the analogy of the road map a bit. As is well known, on any good road map, some of the lines representing highways will be highlighted, thicker and easier to read than others. These are the main highways, the well-travelled routes, the basic freeways of transport. Likewise, in a scientific classification of local species, some organisms will be highlighted in that they are more highly distinctive than other species in the area. They gain their *distinctiveness* as a consequence of the larger numbers and relative weights of the morphological, and to some extent, behavioural characteristics that separate them from organisms in the area. In comparison with other species of the local fauna, these distinctive taxa are at once relatively *homogeneous* (in terms of their internal biological diversity) as well as relatively *isolated* (in terms of the "... size of gaps [separating them from] adjacent taxa" (Sneath and Sokal 1973:291). (My colleague Paul Kay says that such species suffer from a high degree of "phylogenetic loneliness"). Such perceptually distinctive organisms should be the best candidates for names in any ethnobiological system of classification. Ironically, Hunn was the first to make this observation in print. Other things being equal, he stated "... the probability that a group of organisms will be recognised and named is directly proportional to

the [distinctiveness] of [that group] as determined by scientific taxonomic analysis" (Hunn 1977:72, cf. also Hunn 1979).

Can taxonomic distinctiveness be defined? For our purposes, a good operational definition is relatively straightforward and can be read directly from the structure of the biological taxonomy of the species in any local habitat. In general, species that occur as members of locally monotypic genera should be perceptually highly distinctive in that there are no congeners (i.e. species of the same genus) with whom they might be confused. Monotypic genera within monotypic families should be even more distinctive. These are the isolated beacons of biological reality that literally cry out to be named. (Figure 2).

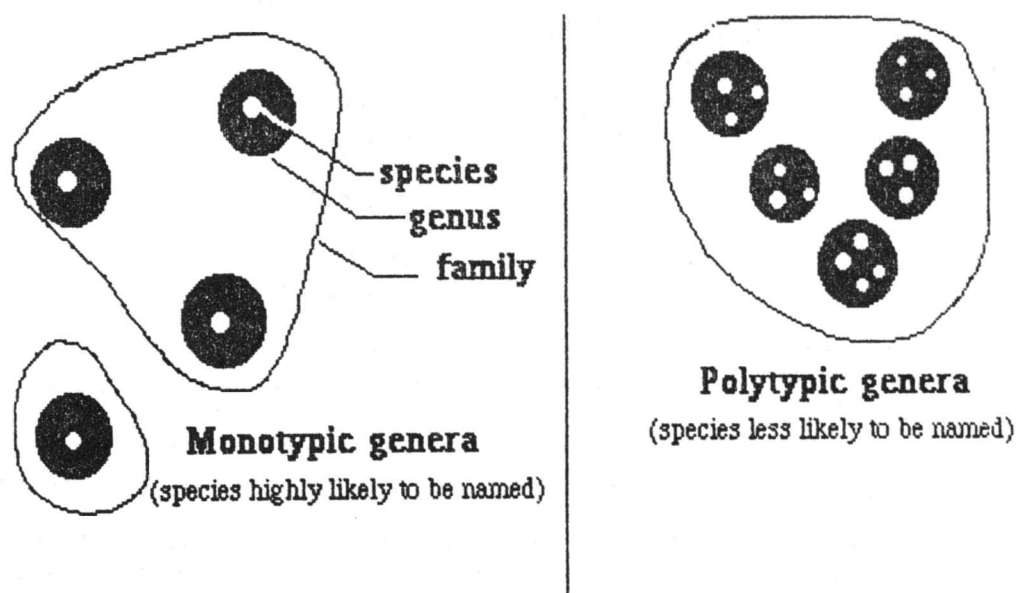


Figure 2. Schematic representation of taxa indicating likelihood of being recognised linguistically in any ethnobiological system of classification

We now have in hand the makings of an experiment that is relevant to the utilitarianist/intellectualist debate on the bases of ethnobiological classification. If one assumes that monotypic genera are perceptually highly distinctive, then the following hypothesis is suggested; it is a simple hypothesis and is divided into two parts:

1. *If a scientific genus, x, is monotypic, it will be recognised in the folk system by an indigenous name,*
2. *The indigenous name will be restricted in its meaning to the single monotypic genus, x.*

Our intellectualist hypothesis, in addition to predicting that all monotypic genera will be named, requires that the names be tightly referentially restricted in their range of meaning. If, as we have claimed, monotypic genera are quite perceptually distinct, one should not expect a common folk name to apply to more than a single genus.

THE DATA

One can now set out to determine to what extent the scientific taxonomy can be used as a predictor of the naming characteristics of the folk ethnobiological system. To elaborate a test of our hypothesis, I will draw on data collected among the Huambisa and Aguaruna, two Jivaroan speaking peoples who reside along the Upper Marañón River and its tributaries in the rainforests of the Peruvian montaña. For purposes of this experiment, it will be convenient to restrict its scope to three major vertebrate groups – the mammals, birds and fishes.

Through the collecting efforts of a team of zoologists working with me in an interdisciplinary ethnobiological research project,² relatively complete faunal inventories for the three major vertebrate groups have been compiled. As can be seen in Table 1, the biological diversity of this region of the Peruvian rainforest is high, with over 100 mammalian species, 280 bird species, and 240 species of fish attested in our collections. The mammals are grouped into 76 genera, 56 of which are locally monotypic. Fourteen of these 56 genera are bats and, with the exception of the vampire bat and certain large fruit bats, are rarely if ever observed closely by the indigenous populations in the region. The 280 species of birds in the region are grouped into some 178 genera, 137 of which are locally monotypic. Finally, the 240 fish species are grouped into 89 scientific genera, 65 of which are locally monotypic (see Table 1).

TABLE 1
DISTRIBUTION OF NUMBERS OF MONOTYPIC AND POLYTYPIC GENERA OF MAMMALS, BIRDS
AND FISH IN THE UPPER MARAÑÓN RIVER REGION OF AMAZONIAN PERU.

| | Monotypic genera | Polytypic genera | |
|----------------|------------------|------------------|-------|
| % monotypic | | | |
| Mammals (N=76) | 57 | 19 | (78%) |
| Birds (N=178) | 137 | 41 | (76%) |
| Fish (N=89) | 65 | 24 | (73%) |

Following standard ethnobiological data collection procedures, large numbers of native informants were interviewed in an effort to elicit indigenous names for species of mammals, birds, and fish in our collections. As with all systems of folk biological classification, variation in naming was common. Both lexical (distinct linguistic forms) and phonological (simple phonetic variants) synonymy in the names for different species was a general feature. Detailed knowledge of the animals varied among individuals, with sex and age of informant being the primary factors underlying level of naming competence. Males always knew more names for birds and mammals than did females, and older males had more complete knowledge of the animals than did younger males. Male and female differences in knowledge for the fish species were less pronounced, probably as a consequence of the fact that both sexes share in the harvesting of these creatures and have the opportunity to observe them about equally well. For present purposes, the native name for a particular species is considered to be the name most commonly given in our various elicitation tasks (see Berlin, Boster, and O'Neill 1981 and Boster, Berlin, and O'Neill 1986 for a discussion of methodological procedures).

RESULTS

Recall that our hypothesis has two parts: (1) monotypic genera will be named in the folk system and (2) named genera will be labelled by expressions that are restricted in meaning to just those genera (i.e. not range over a number of genera).

The results relating to the first part of the hypothesis are quite clear-cut. *None* of the monotypic genera of mammals, birds or fish lack a well-established common name, i.e. none are known simply by such general expressions as 'that's just a bird', or 'it's just some kind of fish'. True, as stated earlier, informants will vary in their ability to apply the names of these animals consistently in standard elicitation tasks, some perceptually close genera at times being confused. The general patterns of agreement in our naming data are unambiguous, however, in showing that each monotypic genus does have a common, well-established name. (In two naming experiments, for example, *Daptrius americana*, the common red-throated caracara, was called *yákakau* by 19 of 21 informants on test 1, and by 20 of 23 informants on test 2. Data for the remaining monotypic birds, mammals, and fish showed similar distributions.) All of these creatures are sufficiently distinctive as to require their being recognised linguistically in the folk system.

The second part of the hypothesis concerns the range of application of folk names to monotypic genera. Table 2 addresses this question. Analysis of our naming data indicates that the 57 monotypic mammal genera are grouped into 38 basic Jivaro ethnozoological taxa that I have referred to elsewhere as folk genera (see Berlin 1972, 1973, 1976, 1992, Berlin, Breedlove, and Raven 1973, 1974). The 137 monotypic bird genera are grouped into 114 folk genera and the 65 monotypic scientific genera of fish into 41 folk genera. Clearly, there is no exact correspondence between the number of biological and folk biological taxa in each system. In all cases, there are fewer folk categories than scientific taxa. But how many of the Jivaro folk genera correspond perfectly with recognised scientific genera? The answer is seen in Table 3.

TABLE 2
CORRESPONDENCE OF NAMED JIVARO TAXA (FOLK GENERA) TO MONOTYPIC SCIENTIFIC GENERA OF MAMMALS,
BIRDS, AND FISH IN THE UPPER MARAÑÓN RIVER VALLEY, AMAZONAS, PERU

| | Monotypic scientific genera | Jivaro folk genera |
|---------|-----------------------------|--------------------|
| Mammals | 57 | 38 |
| Birds | 137 | 114 |
| Fish | 65 | 41 |

TABLE 3
RANGE OF APPLICATION OF JIVAROAN FOLK GENERIC NAMES FOR MONOTYPIC MAMMAL, BIRD,
AND FISH GENERA IN THE UPPER MARAÑÓN RIVER VALLEY, AMAZONAS, PERU

| | Folk generic name applied to a single monotypic genus | Folk generic name applied to two to more genera |
|----------------|--|--|
| Mammals (N=38) | 34 (89%) | 4 (11%) |
| Birds (N=114) | 91 (80%) | 23 (20%) |
| Fish (N=41) | 30 (73%) | 11 (27%) |

The pattern shown in Table 3 is striking; for all three vertebrate groups, folk generic names are highly restricted in their range of application to their respective monotypic genera. This is the case for 89% of the mammal names, 80% of the bird names, and 73% of the fish names. These results strongly confirm the second portion of our hypothesis.

It is revealing to examine the exceptions to our hypothesis for it appears that they are readily understandable on perceptual grounds. In several cases, a single folk generic name ranges indiscriminately over a rather diverse set of genera, examples of which are diagrammatically presented in Figure 3.

Cases of underdifferentiated mammal genera are the bats and the two monotypic genera of spiny rats,

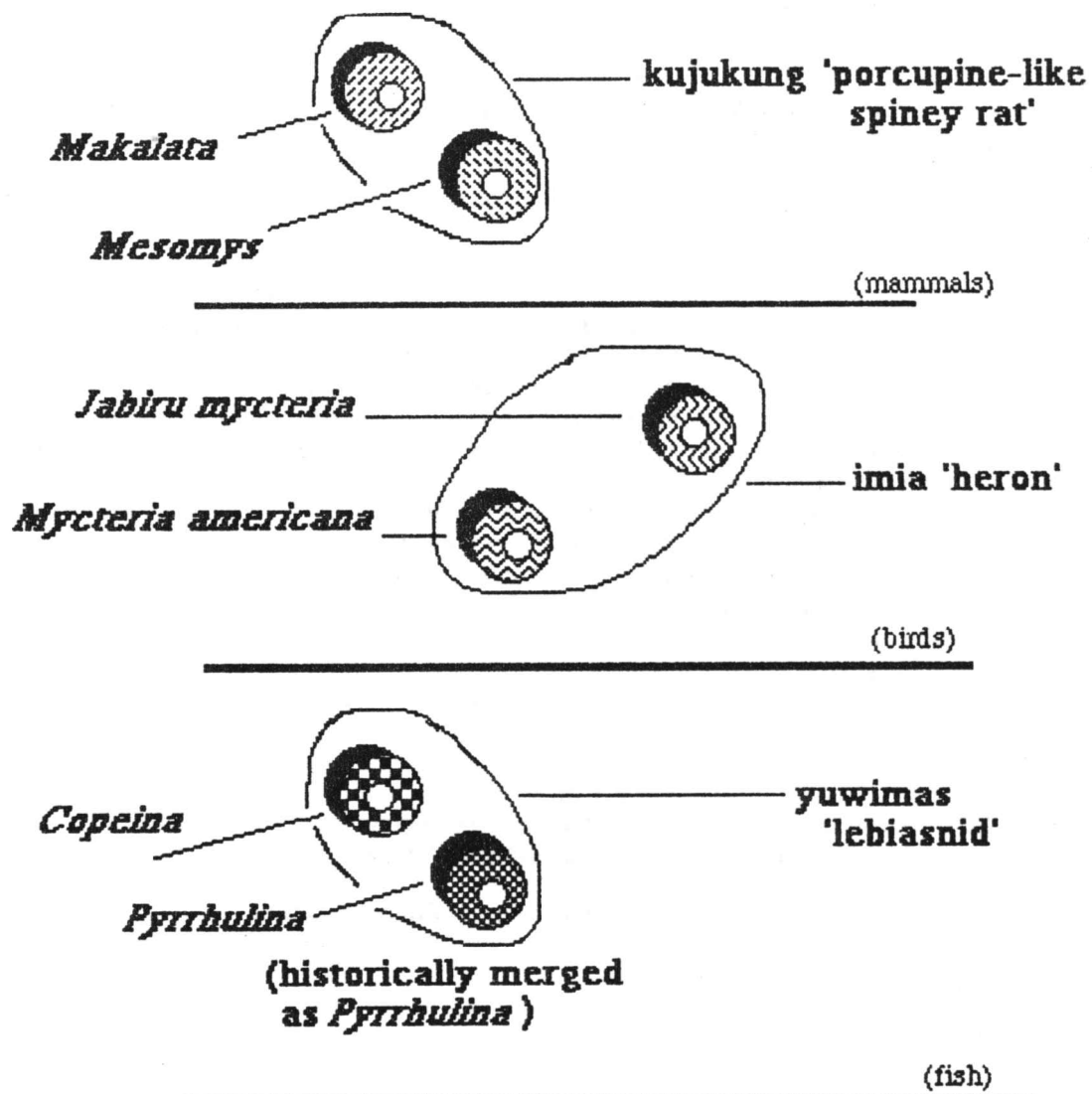


Figure 3. Schematic representation of underdifferentiated Jivaro folk genera of mammals, birds, and fish

Makalata and *Mesomys*. These latter genera are grouped under the folk generic taxon, *kujukung*. Because of their spine-like hair, they are likened to the porcupine, *kuju*, from which their name is clearly derived. Like the bats (*jiincham*), these species are cryptic nocturnal animals and rarely seen by the Jivaro. That they should be lumped into a single category is understandable on perceptual grounds.

Among the birds, the storks are an example of an underdifferentiated folk generic taxon for our Jivaroan informants. Both *Jabiru* and *Mycteria* are treated by the Huambisa as undifferentiated members of the folk genus *imia*. These are very similar white storks, with bare heads and heavy black bills which from a distance are difficult to distinguish. It is not without interest to note that this striking similarity is seen, as well, in the scientific names for the two birds, with the generic name of the American Wood-Ibis, *Mycteria*, serving as the specific epithet for the genus *Jabiru*, i.e. *J. mycteria*, literally 'Mycteria-like Jabiru'.

Finally, among the fish, one can cite the two lebiasinid genera *Copeina* and *Pyrhulina*. Here, the folk generic term *yuwimas* applies equally to both genera. These two genera are quite closely related and, until recently, were joined in the single genus *Pyrhulina*. Their separation today is justified solely on the basis of dentition: *Pyrhulina* has two complete rows of teeth in the upper jaw while *Copeina* shows but a single row. This single character, of course, is not visible to the Huambisa who base their grouping on overall gross morphology.

More commonly, when a Jivaro folk genus refers to two or more scientific genera, the folk genus itself is further divided into sub-classes that can be called folk species, as seen in Figure 4.

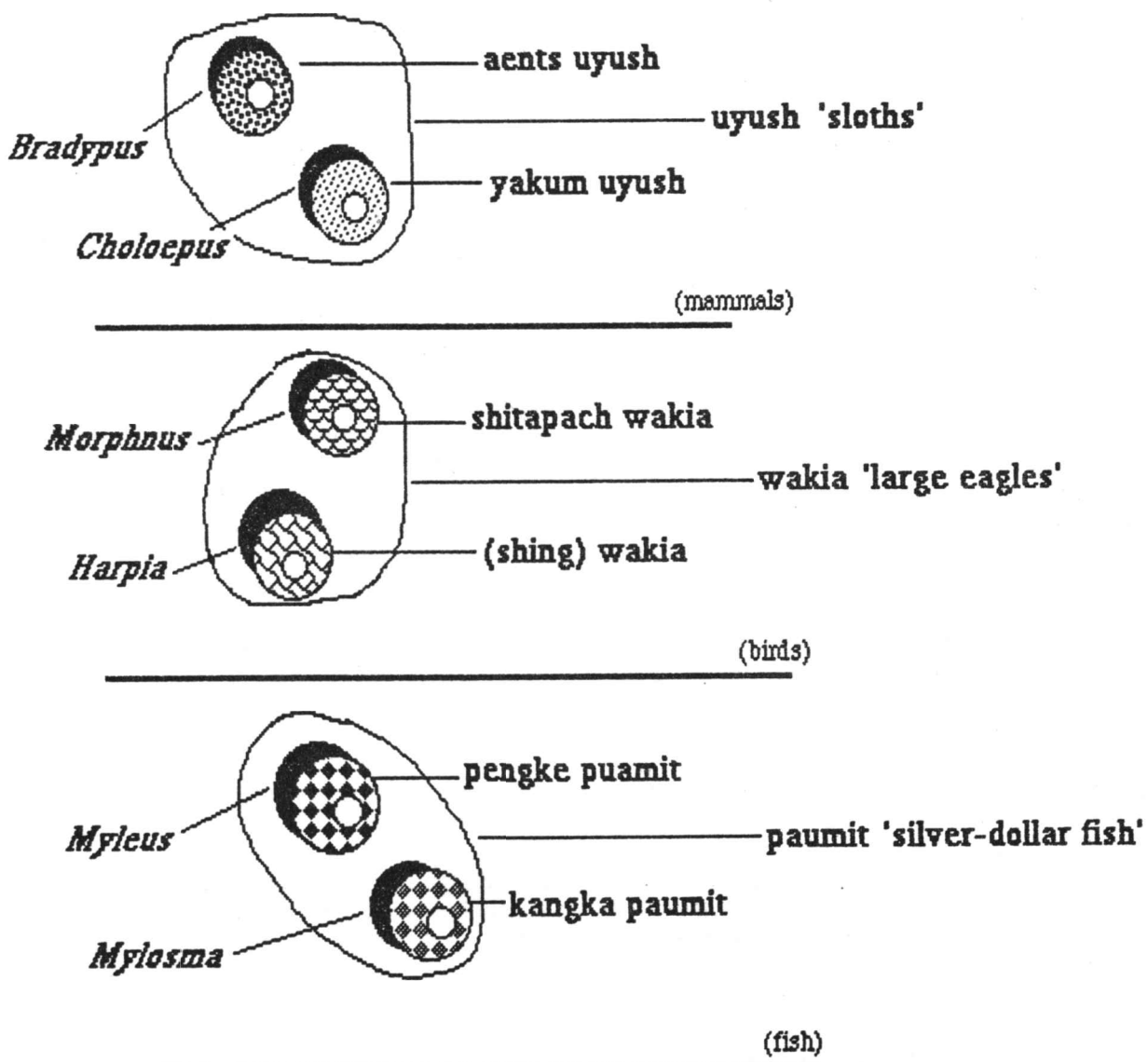


Figure 4. Examples of Jivaro polytypic folk genera of mammals, birds, and fish indicating nomenclatural recognition of monotypic biological genera

Thus, while the Aguaruna apply the term *uyúsh* to both the two- and three-toed sloths in the area, *Choloepus* and *Bradypus*, they nonetheless recognise the differences between these genera in their ethnobiological system of classification as indicated by the two folk specific names *aents uyúsh* (lit. 'man sloth') for *Bradypus* and *yakum uyúsh* (lit. 'howler monkey sloth') for *Choloepus*.

In a comparable fashion, the Huambisa refer to both the crested and harpy eagles (*Morphnus* and *Harpia*) with the folk name *waikia*. In contexts where the two must be distinguished, the former is called *shitapach waikia*, lit. 'lesser *waikia*' in contrast with *shing waikia* 'true or genuine *waikia*'.

A final example among the fish is seen in the treatment of the characid silver dollar fish genera *Myleus* and *Mylosma*. In the folk system, both genera are grouped in the folk genus *paumit*. The included scientific genera are nonetheless recognised by the more specific terms *pengke paumit* 'true *paumit*' and *kangka paumit* 'gross-bodied *paumit*', and are distinguished primarily on the basis of relative size.

These nomenclatural patterns provide us with clues about the size of the gap that actually separates locally monotypic genera. One would predict that mammalogists will judge the two sloth genera to be closer morphologically, than, say, any of the monotypic genera of anteaters in the region, with whom the sloths are most closely related phylogenetically. Ornithologists would, I predict, note that the crested and harpy eagles are closer perceptually to one another than either is to *Spaetustyrannus*, another locally monotypic hawk. And ichthyologists would not dispute that the readily perceptual differences separating *Myleus* and *Mylosma* are many fewer than those that separate two other monotypic genera, say *Paragoniales* and *Salminus* of the same group of characid fishes. Although I do not want to push too strongly on this point, it may be that in some highly predictable ways, the nomenclatural properties found in the folk system provide a more accurate guide to the natural affinities among groupings of locally monotypic genera than does the western scientific system itself.

CONCLUSIONS

In this paper, I have presented data that bear on the ethnobiological debate concerning the bases of ethnobiological classification. I have shown that one can, to a large degree, predict which species in the three major groups of vertebrates in a particular localised habitat will be named in the folk system solely on the basis of a knowledge of the structure of the taxonomic classification of these species in the western Linnaean system. That this should be so supports the assumption that both systems recognise those portions of biological reality that are most distinctive in terms of their gross, overall observable characteristics. Furthermore, these data support the view that ethnobiological classification, and to a large degree, modern scientific biological classification, is motivated primarily by human beings' intellectual efforts to codify linguistically the biological reality that confronts them. To do otherwise would be, it would seem, difficult and highly improbable.

The utilitarian might respond, of course, that the most distinctive creatures in any particular region are precisely those that will be found to be highly significant in terms of their cultural importance. As Hunn has stated, "Biologically natural categories. . . will tend strongly to be categories *useful for many human purposes*" (Hunn 1982:834, emphasis added). Aside from the fact that such a view is logically *ad hoc*, it is not supported empirically. More than half of all of the mammals given linguistic recognition by the Aguaruna (and Huambisa) serve ". . . no direct use either as food or for material goods" (Patton, Berlin, and Berlin 1982:124, see also Berlin and Berlin 1983). Of all of the edible species of mammals, only 11 contribute 1% or more toward total animal consumption and in no case does the total rise to more than 9%.

Birds play an even less important role. Some 30 to 40 bird species, mostly the larger game birds, contribute an average of 8% by weight to all animals consumed, but none of these species contributes more than 3%. By far the most dietarily significant bird is the introduced chicken.

Fish are more important in Jivaro diet than either birds or mammals or the two combined, contributing more than half of all animal biomass consumed. Nonetheless, very few scientific genera account for the majority of fish consumed. By far the largest contributors are a few genera of Loracariid armoured catfish, several large pimelodid and doradid catfish, and a handful of species of characids (see Berlin and Berlin 1983 for a detailed statement of the relative contribution of animals to diet among the Aguaruna and Huambisa).

The majority of vertebrate animals known to the Jivaro cannot be shown to have any immediate utilitarian importance, assuming that it might be possible to develop some direct measurement of utility. Such a measure, by the way, is critically absent from the utilitarian program. Utilitarianists, however, are not daunted by problems of assessing direct importance. As Bulmer once argued, in a rare neo-functionalist statement,

If one sees individual plant and animal categories solely in their direct relationship to man, there are many which appear irrelevant, neither utilised nor noxious. However, if the relationships between different kinds of plants and animals are recognised as relevant, then a great deal of additional forms will

very usefully be identified and classified. . . [Since] animals exist in significant relationships with other animals and plants, there is a considerable impetus to classify these forms also (Bulmer 1974a:12-13).

Surely, if one is allowed to define utility in such an open-ended fashion, counter examples are ruled out on *a priori* grounds. In this neo-Malinowskian world, plants and animals are classified since, by definition, all of them are functionally related to one another in ways as to make their linguistic recognition in the ethnobiological system adaptive. The circularity of this crude ecological argument makes it, in the final analysis, untestable, a view that Hays (1982) pointed out several years ago. Such a tautological position reduces ethnobiological classification to something comparable to a classic Rube Goldberg cartoon, where a character finally gets a cup of coffee by indirectly manipulating a complex apparatus of wires, multiple levers, pulleys, and toggle switches.

The general dimensions of the intellectualist/utilitarianist debate should now be clear. It seems that one need not invoke utilitarian motivations as the bases of ethnobiological classification any more than one would argue that Western Linnaean taxonomy was erected for purely pragmatic reasons. As Simpson and others before and after him have stated, ". . . readily recognisable and definable groups of . . . organisms do really occur in nature" (1961:57). Both the western and folk systems of biological classification will record the fundamental affinities among the differences between organic beings. Both will name the most distinctive groupings of organisms in any particular habitat. Both will act as codifiers of nature's inherent plan.

NOTES

1. This paper is dedicated to the naturalist/ethnobiologist Ralph N.H. Bulmer, who perhaps more than any other single individual set the standards of field research, evidence, and analysis that have led to modern ethnobiology as a discipline. While recognising the importance of social and cultural factors in ethnobiological classification, he never faltered in this stance on the centrality of the perceptual bases of ethnobiological classification and its natural taxonomic structure. The title of my paper derives, of course, from Bulmer's classic characterisation of the significance of natural taxonomy as the foundation of folk systems of biological classification (Bulmer 1970). This important paper appeared at a critical historical juncture in ethnobiology and helped to codify the emerging reorientation of anthropology away from its traditional relativist perspective. Unfortunately, the relativist view in the forms of so-called post-modernist interpretive social theory, a fundamentally narcissistic and scientifically nihilistic position, appears to be re-emerging as a new force in anthropology, one that I am certain Bulmer would have moved to counter.

The sustained collaboration of James L. Patton (mammalogist), John P. O'Neill (ornithologist), and Camm C. Swift (ichthyologist) as partners in the Second Ethnobiological Expedition of the University of California to the Upper Marañón River is much appreciated. I am grateful for the comments and collaboration of Elois Ann Berlin for work on the nutritional aspects of Jivaro ethnobiology, and to James Shilts Boster for his conceptual and quantitative contributions to analysis.

Finally, in spite of Eugene Hunn's current position on what I believe to be an overriding and unfounded significance of utilitarian factors in ethnobiological classification, he was the first to present a formalised perceptual model to account for the observed regularities found in all systems of ethnobiological classification.

2. Research among the Aguaruna and Huambisa has been generously supported by the National Science Foundation (BSN7-17485), the Language Behavior Research Laboratory and the Museum of Vertebrate Zoology of the University of California, the Museum of Natural Science of Louisiana State University, the Missouri Botanical Garden, the Los Angeles County Museum of Natural History, the National Fish and Wildlife Laboratory of the U.S. Fish and Wildlife Service, and the Center for Latin American Studies, University of California, Berkeley.

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PART III

SOCIAL AND SYMBOLIC SYSTEMS



WHEN ALL ELSE FAILS: SUICIDE AMONG MARING WOMEN

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On many occasions I had the opportunity to visit Ralph Bulmer in the field or at Simbai or at the University of Papua New Guinea or the University of Auckland. During these visits we discussed the causes and consequences of the high mortality rates found among the Maring speaking people in the Simbai Valley. Particularly troubling was the high mortality rate among girls and women up to the age of about 45 and the consequent shortage of females in the population. This imbalance was profound for people aged 20 to 35. In 1976 when I left the field there were twice as many men as there were women. We attributed this to women's low status, which translated to poorer diets and harder work. Rappaport and I (1976) noted that women's work in caring for the sick and the dead put them at greater risk of infection. A cause of female mortality that I did not consider until some time later (Buchbinder 1986) was suicide.

For some people at some times in some societies suicide has been considered a reasonable and even honourable way to die. For most people throughout most of the world, however, suicide is not condoned. Its societal and personal costs are too high. Suicide is an act that invariably has a profound effect on both the victim and the survivors. As with any death, social relations within the surviving community must be redefined. The death creates a gap or void. To most people in most communities it is such an unlikely or repugnant act that it must be explained. Why would a person take their own life? Were they mentally deranged? Were they bewitched? Is someone to blame? Is society to blame?

A suicide death resembles a death due to an acute disease process or an accidental injury. It is usually not anticipated. In retrospect survivors often say there were telltale signs. Because it comes as a surprise its societal ramifications are likely to be dramatic.

Attempts have been made to explain suicide on both the societal and the individual level. Much early sociological research on suicide, dominated by the thinking of Durkheim (1897) concentrated on the role of social control and of social integration. For Durkheim there are three different motives for suicide. Egoistic suicide is related to the loosening of family or social controls, anomic suicide is caused by social adversity and economic hardship, while in altruistic suicide individuals kill themselves for some higher purpose. Jeffreys (1952) introduced the term Samsonic suicide for those that were motivated by considerations of revenge. He distinguished two types of Samsonic suicide: that based on a belief that the ghost of the suicide could execute vengeance, and that based on societal reprisals against those considered responsible for precipitating the event.

Psychodynamic explanations of suicide are varied and complex. Suicide has been viewed as aggression directed towards oneself, as an attempt to make others feel guilty, or as a result of overwhelming feelings of helplessness or hopelessness. It is acknowledged that social and individual factors influence each other but there has not been much systematic research on this interaction. Suicide is the mortality of depressive illness. Psychotics in the West have much higher suicide rates than nonpsychotics and those with depressive psychosis or involutional melancholia have the highest rates (Monk 1986).

In a recent review of suicide in Papua New Guinea Pataki-Schweizer (1985) cites crude suicide rates for several Highlands societies. These range from 8.5 to 17.0 per 100,000. These crude rates are in the middle of the range of reported worldwide suicide rates. They are remarkable, however, in their component age and sex specific rates. Most suicides in Papua New Guinea occur among young people, i.e. those aged 15-34, and most of those young people are women.¹

Among one of the groups cited, the Huli of the Southern Highlands (Smith 1981), the rate for both sexes aged 15-45 was 34/100,000. However, the rate for women in that age group was 61/100,000, nine times higher than the male rate. Interpersonal conflict, particularly with husbands, is likely to be the precipitating event.² The Oksapmin near the Indonesian border have a crude suicide rate of 72/100,000 (Weeks 1981); sex specific rates are not reported.

High female suicide rates are very unusual. In most of the world, and particularly in other Pacific Island nations, young men are more likely to suicide. In the developed world women attempt suicide more frequently than do men, but men are more often successful. It is not clear whether the sex difference is due to motive or to method,³ but both are culturally determined.

Johnson (1981) reported a remarkably high crude suicide rate of 300/100,000 among the Gainj people in the Taugi River Valley in the Madang district near Simbai. For women in this group aged 20-49 the rate is 1,200/100,000. In 1977-1978 suicide accounted for 10% of all deaths among the Gainj. Among women aged

20-49 suicide accounts for 57% of all deaths, and is by far the leading cause of death. Johnson notes that the Gainj consider suicide to be a tragedy, but not a shameful or criminal act.

Female suicide can be seen as growing out of a gender system that requires male dominance, but that offers women in return a definite assurance of protection by dominant males. When women keep their part of the gender bargain but men do not, suicide becomes a possible course of action. It will, at once, remove the woman from an intolerable situation and visit a lasting revenge on her defaulting husband (Johnson 1981).

A number of anthropologists have been impressed with the high rates of suicide among women in the New Guinea Highlands and have tried to explain them. Consideration is given to social control and Samsonic motivation.

In his 1979 paper Healey has reviewed the literature on female suicide in the New Guinea Highlands. He makes reference to the work of Berndt, Koch, Strathern, and Reay. Berndt (1962, 1972) describing conditions under which suicide occurs among the Kamano, Usurufa, Fore and Jate of the Eastern Highlands noted that marital discord was the most common reason for suicide, and was practised mainly by women. Shame is also mentioned as a motivating factor. Samsonic intent as in punishing an unkind husband may play a part but Berndt does not dwell on this, although he concedes that there may be a desire to punish those who cause shame.

Koch (1974:74-6) writing about the Jale' of Irian Jaya is more explicit about Samsonic aspects of suicide. He asserts that one response to conflict between individuals is avoidance, and "suicide is the most extreme manifestation of avoidance". Further, "the Jale' believe that such suicides mean to hurt the guilty, who afterward suffer feelings of regret and self-reproach". The dead person's kin demand compensation for the death. There is little fear of the deceased's ghost. Most Jale' suicides are women or old men.

According to Strathern (1972) among the Melpa most suicides are married women. Suicide typically occurs when there "is dual alienation from both husband and home kin" (Strathern 1972:256). This happens when a woman who attempts to return to her natal home after quarreling with her husband is sent back to him by her agnates. She may then "hang herself out of frustration and revenge". There is a belief among the Melpa in the ability of the ghost of a suicide to take Samsonic revenge on survivors. Strathern (1972:256, 281-3) regards most suicides or threats of suicide among the Melpa as acts of self-help by women, who lack the judicial ability to influence the relationships with their agnates and husband's kin to their own advantage.

For Kuma women suicide is a response to marriage (Reay 1959:177-9, 202-3). Most suicides and attempted suicides occur among young brides. They are an expression of rebellion against the loss of their considerable adolescent freedom that occurs at marriage. Most Kuma girls are married against their will. Typically, suicide follows a bride's repeated unsuccessful attempts to return to her natal home. Reay (1959:202) concludes that "the frequency of actual and attempted suicide is perhaps the most dramatic evidence of true strain in the traditional life" and that "a high suicide rate can be expected when social unity is sought by over-rigid regulation at an excess cost of self-expression". Women find it difficult to accept the duties and obligations imposed upon them by marriage.

Healey (1979) notes that Reay, unlike Strathern, seems to regard suicide by women not so much as an act of self-help when men refuse to accede to their desires, but rather as an escape from intolerably regulated relationships.

That is, Kuma suicide for Reay seems to be an attempt to escape from social relationships deemed unsatisfactory by women, rather than as an attempt to effect their transformation to a state in which women regain some measure of autonomy and control, as is the case of the Melpa. Yet the situations in which suicide often occurs seem much the same in both societies (Healey 1979).

Healey then goes on to deny rigid social control in the highlands and to discuss women's dubious power over men through the denial of services and the threat of sexual pollution. He then argues that too much attention has been given to suicide as the last act of a desperate and powerless woman. Rather, he feels that one can view suicide as the first act of a woman who has unilaterally assumed a transcendent power, above that accorded by her inferior status. If this view is taken then he states the act is likely to have dramatic impact on the actors' conceptualisations of social structure. This may be true but it can be of little solace to a dead bride. It may be her first act but it is also her last and only act. Only among the Melpa, of all groups discussed here, is there even a possibility of ghostly revenge.

Ethnographers working in New Guinea have been slow to recognise or acknowledge the extremely low status of women and their consequent misery. Much has been written about differing sex roles and sexual tension (Brown and Buchbinder 1976), but no one has seriously addressed the issue of the complete powerlessness of women in most Highland New Guinea societies. It is as if this lack of power has not been

seen or recognised. Women work like pack animals to provide food and firewood for their families. They may be beaten by their spouses. Since the introduction of steel tools, their relative status has dropped as their work load has remained much the same while that of men has dropped. One manifestation of this low status is the higher death rate suffered by all females up to the age of 45. This high death rate is the result of poor diet, hard work, more exposure to infection and possibly suicide. Only a small part of it is related to childbirth. Papua New Guinea is one of the few countries in the world where there are more men than women. The others are all on the Indian sub-continent, where women also have extremely low status. Elsewhere (Buchbinder 1976), I have argued that woman's status has deteriorated still further since contact with the West.

Women in Highland New Guinea societies traditionally have no independent identity. They are defined in terms of their relationship to men. Early in their lives they take their identity from their father and brothers, later from their husband. Young women, those who have recently entered a marriage, are in a particularly weak position. They have loosened their ties with their natal family, but not cemented them with their new family. Older women, particularly widows with grown sons, may acquire some degree of autonomy, but it is usually small. Women are not allowed to speak publicly or to make their wishes known. Elder postmenopausal women may occasionally speak publicly, but they are usually ridiculed for their efforts.

Young girls work alongside their mothers in the gardens and care for younger siblings, while boys run free. For a brief time adolescent girls may have some freedom. Their work load becomes minimal, they preen for and participate in courtship ceremonies. This is the best time of their lives, they become plump. They are healthy and happy. They believe that they have the ability to select their own mate. If their selection coincides with that of their male kin there is no conflict. But if it does not then the realisation of their total lack of power dawns and the stage may be set for suicide. In all accounts of female suicide in the New Guinea Highlands the hopelessness and powerlessness of the victim is apparent. Motives of revenge may be present, but they are probably secondary. Only the Melpa believe that the ghost herself can extract revenge. In all other societies, the deceased must depend on relatives she could not depend on in life to exact retribution.

I did field research in the Simbai Valley of Highland New Guinea among Maring speaking people during the decade between 1966 and 1976. The Maring share a border with the Kalam people among whom Ralph Bulmer worked. This research was begun shortly after the Maring had been contacted by the outside world in 1960 and while much of their life was still lived in the traditional manner.

Some changes had occurred. Warfare had been suppressed. Men had the opportunity to work on coastal plantations, where they earned money, bought goods and learned to speak Pidgin. Women's life was much the same. They had no new options. Possibly they had more work with the men away. Their mortality relative to men's increased.

Among the Maring, a woman may attempt to select a mate by eloping to him. Frequently, because men fear sexual contamination, the attempt is futile. The man of her choice, while flattered by her attention, is so fearful of losing his youthful vigour that he rejects her. The experience is humiliating for the woman. She is passed from her chosen mate to other young men, all of whom may reject her. Eventually, an older man with some relationship to her family will take her home to her kin. Even if she is not rejected, she may discover that her father or brother had other plans for her, and they may come and take her away. In more recent times they may initiate a "court" designed to make her return to fulfil prior obligations.

Most men will claim that their marriage results from their being chosen by a woman, but this is probably not true. Many marriages are arranged between men who feel they have something to gain by becoming affines, and the preferred form of arranged marriage is sister exchange. Fathers and guardians may also attempt to arrange marriages for their daughters and female wards. These arranged marriages are also traditionally used to cement social relationships among men. As the amount of bride price escalated after contact many men tried to arrange economically advantageous marriages. Young women were told that it was their duty to marry the man who would provide the biggest bride price. Women tend to loathe these arranged marriages for they are frequently given to men who are considerably older than they.

While men fear dissolution, premature ageing and death from contact with women, women have similar fears about contact with older men. For Maring women a smooth hairless unwrinkled skin is a desirable trait in a mate.

Healey learned of six suicides in a Maring community of about 300, over a three year period. All of them were women. This produces an astounding crude annual rate of 666/100,000. I know of three more female suicides in nine different Maring communities, numbering about 2,000 people over a 10 year period. This converts to a crude rate of 15/100,000. The difference in crude suicide rates reported by me and by Healey most likely reflects the instability of rare events in small populations rather than any real difference. However, it is possible that these events were less often reported to me. It is also possible that the suicide rate increased with time as women's status declined. Remember that Johnson (1984) reports similarly high rates during the same time period for the Gainj who live just across the Simbai Valley. Healey (1979) describes in great detail

one of the six suicides. I will take this opportunity to describe the suicide that occurred while I was with the Maring.

One wet morning in January 1969 a very excited man from Tsembaga, the village next to mine, arrived and told us a most amazing story. One of the men in his village had just beaten his mother. She was seriously hurt. This was extraordinary. Maring men do not beat their mothers. Instead they take great pains to assure that their mothers are comfortable and happy. A mother's ghost is believed to be capable of causing sickness or death, particularly if she feels that she has not been adequately cared for during her old age. Sons therefore usually take special care not to offend their mothers.

Why had this man beaten his mother? Why had he deliberately risked his life? I was told it was because his mother was constantly nagging his wife. This was also odd. A woman might not approve of her son's marriage, and she might resent the presence of a daughter-in-law who would compete with her for her son's affection and labour. But if she has a complaint, it is usually voiced to the son, not to his wife. Maring mothers usually do not nag their daughters-in-law. In an argument, a man would rarely take the side of his wife over that of his mother. He would never strike his mother.

Why was the mother nagging her daughter-in-law? Because the girl would not leave her side. She worked in her mother-in-law's garden, she cooked at her hearth, she slept in her house. She would not stay with her husband, she would not garden with him, she would not cook for him. This was not completely unusual for a new wife. Brides could be shy and would then take refuge with their mothers-in-law. Young husbands could be equally shy and tacitly approve of this arrangement. They might even take this opportunity to absent themselves for two years and go to work on a coastal plantation, claiming to be too young to consummate their marriage. In this case, however, the husband was not young. The young woman's avoidance behaviour had persisted for many months and the husband's mother became increasingly adamant that the girl should assume her role as wife.

The marriage was a sister exchange marriage, a traditional form of marriage favoured by men but loathed by women. Women much preferred to pick their own mates. Men felt they could control women and their own fortunes if they could arrange their sisters' marriages. These marriages were especially loathsome for the women if, as in this case, the man was much older. Although the marriage was unacceptable to the wife she could not leave, for to do so would be to shame her own brother, and this was unthinkable. The brother-sister relationship was supposed to be very close and full of respect, at least by a woman to her brother.

Normally relations between a brother and sister are warm and mutually supporting, but if a man feels that he can advance his position by arranging a strategic marriage for his sister he will not hesitate to do so, frequently without even consulting her about her wishes. Because most Maring women do not want to shame their brothers, they will try to make the best of the situation. But if the husband or his kin are insensitive and try to coerce the bride into accepting her new status before she is ready, she may commit suicide.

The situation had become intolerable in the case of this young woman. She would not consummate her marriage, she could not return to her own family, and her mother-in-law who had initially been supportive was becoming more and more insistent that the marriage be consummated. Daily she was told that she was shirking her duty, that she was ungrateful and worthless.

Caught in this terrible bind, the young woman secretly made a stout rope, and early that morning she left the house of her mother-in-law and hanged herself from a tree near a frequented path in the settlement. Her body was found shortly after dawn, and her husband in his rage and grief at the loss of his bride severely beat his mother, blaming her for the death.

Two other young women killed themselves during my stay among the Maring. Their stories are different in detail but each involved a young bride in an arranged marriage. Many people knew of similar incidents in the past.

The Maring, like many New Guinea Highlanders, worship their ancestors and believe that the ghost of an ancestor can influence the living. Frequently ghosts are believed to cause sickness or death. The ghost of one's parents, particularly the ghost of one's mother, is most feared. This fear usually ensures that living mothers are treated with respect and consideration. Sacrifice is usually only offered to appease male ghosts. A husband's ghost can cause trouble to a wife if she does not perform requisite funeral rites and to any man that she might marry before the end of the formal two year mourning period. But I have never heard any fear expressed of the ghost of a young woman. She is as powerless in death as she was in life.

The contexts in which women commit suicide throughout the New Guinea Highlands are remarkably similar. Unwillingness to marry or conflict in a marriage are usually antecedent. Other forms of interpersonal conflict may occasionally trigger the event. It is clear that in these cases suicide is the last desperate act of a helpless and unbearably unhappy woman. The argument that these suicides are motivated by desire for revenge is almost beside the point. This may indeed be a part of the motivation, but it does not help the victim. There will indeed be a perturbation in the social fabric after her death. But it will be temporary, and life will go on much as it did before without her. Her suicide will not change the conditions that led to it. Other women

will be forced to the same action. To understand these suicides, we really need to understand the depth of the misery and hopelessness of the lives of women in these traditional societies.

NOTES

1. Poole (1985) reports a different pattern of suicide among the Bimin-Kuskusmin of the eastern Mountain-Ok region of the West Sepik Province of Papua New Guinea. Here he claims that suicide accounts for about 10% of all deaths, that two thirds of all suicides are by males. He also states that women attempt suicide twice as often as they actually succeed.
2. Smith (1981) makes the point that the method of suicide among the Huli (and indeed throughout the New Guinea Highlands), hanging, is so lethal that there are very few failed suicide attempts.
3. Men are more likely to use more violent and lethal methods such as guns, and leaps from a great height. Women use less violent and less lethal means such as overdoses of sedatives.

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PART IV

LINGUISTIC AND TEXTUAL ANALYSIS



ON HARUAI VOWELS

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1. INTRODUCTION

The Haruai (Wiyaw, Waibuk) language is spoken by some 1,000 Highlanders in the Western Schradars, in the Mid-Ramu District of Madang Province, Papua New Guinea.¹ Haruai and the closely related Hagahai (including the Aramo dialect) and Pinai together form the Piawi family; while the Haruai have close contacts with the Kobon, whose language belongs to the Kalam family, and have borrowed much vocabulary from Kobon, there is no evidence of genetic relatedness between the Piawi and Kalam families (Comrie 1988). Haruai phonology, like that of languages in the Kalam family (see especially Biggs (1963), Pawley (1966:33-7) for Kalam), belongs to an areal type referred to as the Sepik-Ramu type (Wurm 1982:210), characterised by a relatively large number of consonant phonemes and a relatively small number of vowel phonemes, many instances of phonetic vowels being readily relatable to more abstract representations not containing these vowels. The relative richness of the Haruai consonant system can be seen, for instance, in the existence of a phonemic opposition between palatal(ised) and plain consonants, e.g. between /j/ and /d/, between /ɸ/ and /b/, and of a phonemic opposition between labialised and plain velars, e.g. between /gʷ/ and /g/.

At first blush, Haruai might appear to have a relatively rich vowel system too, with the following set of distinctions: *i*, *ɨ*, *u*, *e*, *ə*, (hereafter transcribed *ö*), *o*, and *a*, making a seven-vowel system as in Table 1. Some of these seven putative phonemes have variants whose allophonic nature is readily recognisable: Thus *ɨ* is fronted in the environment of palatal(ised) consonants and backed and rounded in the environment of labialised consonants, though not to the extent of merging with *i* or *u*, at least in slow speech. Further, *ö* and *a* are often fronted after palatal(ised) consonants (*ö* also before /y/), which in the case of rapid speech leads to overlap between *ö* and *e*. Further investigation, however, shows that three of these vowels (*i*, *ɨ*, *u*) are best analysed as not being distinct phonemes, while for a further three (*e*, *o*, *a*) such an analysis is at least prima facie plausible, though not unproblematic. The seventh vowel, *ö*, must be accorded phonemic status. These vowels are treated in turn below.

TABLE 1: HARUAI VOWELS (PROVISIONAL ANALYSIS)

| | Front | Central | Back |
|------|-------|---------|------|
| High | i | ɨ | u |
| Mid | e | ö | o |
| Low | | a | |

2. *i* AND *u*

In Haruai, the high front unrounded vocoid and the high back rounded vocoid appear in both syllabic and nonsyllabic variants. The distinction between the syllabic and nonsyllabic vocoids is entirely predictable in terms of the phonological environment, according to the following rules:

(a) In word-initial position, the syllabic allophone appears if there is a following true consonant (i.e. excluding glides), e.g. /yb/ [ib] 'name'.³

(b) In word-initial position otherwise (i.e. before vowels and glides), the nonsyllabic allophone appears, e.g. /yör/ [yör] 'place', /yw/ [yu] 'knife'.

(c) Otherwise, proceeding from left to right through the word, the nonsyllabic allophone appears after (phonetically) syllabic segments and the syllabic allophone after (phonetically) nonsyllabic segments, e.g. /dw/ [du] 'go', /yw/ [yu] 'knife', /ywö/ [yuö] 'meat', /möygʷ/ [möygʷ] 'forehead' with the following exception:

(d) The nonsyllabic allophone occurs after a glide preceded by a syllabic segment if there is an immediately following syllabic segment, e.g. /döwyö/ [döwyö] 'cassowary'. There is thus no phonemic opposition between *i* and *y* or between *u* and *w*.

This lack of phonemic opposition also shows up in the form of alternations between syllabic and nonsyllabic allophones. The indefinite clitic =y 'a, some' is syllabic in *hön=y* 'a pig' but nonsyllabic in *nöbö=y* 'a man'. The verb *w* 'go, come' is syllabic in its stem form (used in serial verb constructions and also

as third person singular present) but nonsyllabic in the second person singular imperative *w-ö*, with the second person singular suffix *-ö*.

While I have shown that there is no phonemic opposition between syllabic and nonsyllabic *i/y* or *u/w*, I have not yet considered whether these two phonemes should be grouped primarily with the vowels, i.e. having a secondary nonsyllabic allophone, or primarily with the consonants, i.e. having a secondary syllabic allophone – or conceivably as being underspecified for membership in either of these classes. In fact, there are good reasons for grouping them with the consonants rather than with the vowels. As will be seen in section 3, the true consonants also have syllabic and nonsyllabic allophones, and it is possible to combine the specification of syllabicity of *y* and *w* with the specification of syllabicity of the true consonants. The ‘true vowels’ (i.e. *e*, *ö*, *o*, *a*, whether or not all are analysed as distinct phonemes) do not have nonsyllabic allophones. A further argument is that Haruai has a rule whereby the vowel *ö* is dropped before a following vowel, so that a verb form like *dy-n-ö* ‘go-FUTURE-2SINGULAR’, i.e. ‘you will go’, appears as *dyn* before the quotative particle *e*, the exclamatory particle *o*, or the declarative suffix *-a*;⁴ *ö* is not, however, dropped before *y* or *w*, just as it is not dropped before a true consonant. Finally, in Haruai verb stems can end in most of the nonpalatal(ised) consonants, but not in a vowel; there are, however, verb stems ending in *w*, e.g. *dw* ‘go’, again suggesting that in Haruai this is a consonant rather than a vowel.⁵ The best analysis is thus to assign *i* and *u* as allophones of the consonantal phonemes *y* and *w*, thus removing *i* and *u* from the inventory of vowel phonemes.

3. *i*

Although *i* is phonetically perhaps the most frequent Haruai vowel, it is in all its occurrences predictable (down to the level of free variation). In general, Haruai avoids phonetic consonant clusters. Where two consonants would occur in sequence, or where a word would consist only of a consonant, the phonetic *i* vowel is inserted after the first or (sole) consonant. There are some exceptions to this generalisation: A few consonant clusters are tolerated word-initially, though in free variation with the same cluster split up by *i*, e.g. /*pl-ö*/ ‘shoot-2SINGULAR’, a singular imperative form, may be either [plö] or [pilö]. Some consonant clusters are tolerated (some in free variation with pronunciations having *i*) provided they are both preceded and followed by syllabic segments, e.g. /*nölpö*/ [nölpö] ‘cross-cousin’. However all these ‘exceptions’ to the most general rule are phonetically specifiable, so it remains true that all occurrences of *i* are predictable in terms of the phonological environment, down to free variation. In word-final position, there is some free variation between the presence of *i* after certain consonants and its absence, with the phonetically word-final consonant being usual except in very careful speech or when another word beginning with a nonsyllabic segment follows very closely.

Essentially, what this means is that Haruai consonants have syllabic allophones, e.g. /*p*/ has, in addition to its basic nonsyllabic allophone [p], a syllabic allophone [pi]. This insight enables one to unify the treatment of the three ‘high vowels’ *i*, *ö*, and *u*. Just as *i* and *u* are syllabic allophones of /*y*/ and /*w*/ respectively, so *Ci* is a syllabic allophone of /*C*/, where *C* stands for any true consonant. Point (d) in section 2 above then simply reflects the fact that [yw] and [wy] are permitted phonetic consonant clusters in Haruai when both preceded and followed by syllabic segments.⁶

Just as in the case of *y* and *w*, the syllabicity specifications for Haruai true consonants give rise to alternations. The verb *p* ‘take’ is syllabic in its stem form, i.e. [pi], but nonsyllabic in its second person singular imperative *p-ö*, i.e. [pö].

Before concluding this section, it is necessary to provide some further justification that this analysis of *i* is not just an analytical trick. In a language with a strict phonetic syllable structure of the type CV and where five contrasting vowels are possible in position V, it would always be possible arbitrarily to claim that one of these vowels is nonphonemic, removing it from phonological representations and inserting it by rule whenever a consonant is not followed by a vowel – but the very arbitrariness of the choice of vowel shows that this analysis does not capture a significant feature of the phonological system of such a language (cf. Halle 1970:101). However, Haruai syllable structure is not so simple as that of the imaginary language just mentioned, since it permits initial vowels, as in *öd* ‘wander’, *ab* ‘throw’, *ehöw* ‘no’, and while I have no attestations of *o* (which is rare anyway in Haruai) in word-initial position, it is morpheme-initial in the exclamatory particle *o*; by contrast, *i* never occurs word-initially. On the analysis presented here, this follows automatically, since the syllabicity of a consonant is always represented by a following *i*; if *i* were to be treated as a vowel, it would be necessary to stipulate that it does not occur word-initially. (Since Haruai does not tolerate phonetic vowel clusters within a word, it is not possible to construct a similar argument for vowels occurring immediately after a preceding vowel, though such an argument would in any case merely replicate that for word-initial position.) A further piece of corroborating evidence is that in those cases where Haruai writers have had to write down Haruai words (e.g. names on labour contracts), following the basic spelling conventions of Tok Pisin, they have not provided any orthographic representation of *i*; the absence of an

obvious representation of *i* in Tok Pisin cannot be the reason, since they do attempt to represent *ö* orthographically, usually as *a*. Thus, the analysis of *i* as part of the phonetic realisation of a syllabic allophone of a consonant neatly captures all the relevant facts, some of which are necessarily not captured by analyses that would treat *i* as a vowel in phonological representation.

4. *e* AND *o*

The vowels *e* and *o* are of rather low frequency in Haruai. In a number of instances, they clearly derive from sequences *öyö* and *öwö* respectively, the underlying sequences being retrievable from the morphological structure of the word in question, sometimes also being possible alternative pronunciations. It will be convenient to start with *e*.

A clear instance for the derivation of *e* from *öyö* is in certain future negative verb forms. The future tense suffix in Haruai is *-n* for first and second persons singular and first person plural, and *-ön* for third person singular and second-third person plural; only the *-ön* allomorph will concern us here. In the third person singular, the person-number inflection is zero; in the second-third person plural, the person-number inflection is palatalisation of the last segment of the word. In addition, the future tense suffixes occasion palatalisation of (some, specifiable) preceding consonants. Thus, from the verb *pal* 'hit' we have *pay-ön* 'he will hit', *pay-ön* 'they will hit'. The negative suffix is *-öl*, positioned between the verb and the tense suffix, in which position it is palatalised to *-öy* by a following future tense suffix. Thus we would expect **pal-öy-ön* for 'he will hit' and **pal-öy-ön* for 'they will hit'; in fact the only possible pronunciations are *palen* and *paleñ*. In these examples, then, the underlying sequence *öyö* is obligatorily contracted to *e*. The possibility therefore arises of deriving all instances of *e* from */öyö/*, thus banishing *e* from the phoneme inventory of Haruai. In many instances, of course, there would be no morphological evidence for underlying */öyö/*, but equally no evidence against such analysis, as for instance in the lexical item *höpeb* 'wild' (probably a loan from Kobon *hapeb* 'wild'). Unfortunately, the attempt to derive all *es* from *öyö* does run into some problems.

First, even in instances where *e* does derive morphologically from *öyö*, there is variation in whether the coalescence is obligatory, preferred, or dispreferred. In the negative forms cited above, it is obligatory. In corresponding affirmative forms of the verb *nöl* 'give', the same morphophonemic sequences arise, but here, although the contraction is strongly preferred, uncontracted forms are also acceptable, e.g. *nen* or *nöy-ön* for 'he will give' (morphophonemically {*nöl-ön*} 'give-FUTURE', with the future tense suffix *-ön* causing palatalisation to give *nöy-ön*. When the clitic postposition *=yöŋö* is attached to a noun phrase ending in *ö*, the coalescence is preferred, though not obligatory, i.e. alongside *hön=yöŋö* 'with the pig' we have both *nöbö=yöŋö* and (preferably) *nöbeŋö* 'with the man'. In the negative of the second person singular present tense, it is preferred not to contract the *öyö*, although it is possible to do so, at least in rapid speech, i.e. *dw-öy-ö* 'go-NEGATIVE(-PRESENT)-2SINGULAR' 'you do not go' is preferred to *dw-e*.⁷

Secondly, there are some lexical items that occur with either only *öyö* or only *e*, in the same environment (though it is possible that there may be further idiolectal variation that I failed to uncover). Thus the word *söyö* 'lizard' does not have a variant **se*, except perhaps in very rapid speech, while *we* 'rotten' does not have a variant **wöyö*, indeed it forms a minimal pair with a hesitation particle *wöyö*. Despite this, there is still some evidence for deriving *we* from */wöyö/*: When the normal suffix *-a* is added to *we*, the resulting form is [wöya], or perhaps [weya], since the *ö/e* opposition is neutralised before *y*; this is readily accounted for in terms of an underlying */wöyö/* and regular deletion of *ö* before a following *a*.

One item that always appears in the form *e* is the quotative particle. Before this particle, the vowel *ö* simply drops, so that, as noted above, *dy-n-ö* 'you will go' appears as *dyn* before *e*, as in *dy-n e r-öŋ-a* 'go-FUTURE(-2SINGULAR) QUOTATIVE do-PAST:3SINGULAR-DECLARATIVE', i.e. 'he said: I will go'. If quotative *e* is analysed as underlying */öyö/*, one might expect the sequence */ö+öyö/* to give [ayö] by *öö*-coalescence (see section 5), if *öö*-coalescence is ordered before *öyo*-contraction, but in fact **dynayö röŋa* is quite impossible. Either the inverse rule ordering must be posited (which I cannot exclude), or this *e* is not derived from *öyö*.

Overall, then, the question of how many instances of *e* should be derived from *öyö* encounters a number of conflicting answers. In some instances *e* clearly must be derived from underlying */öyö/*, for morphological reasons, though even here complex conditioning is required to show when the coalescence is obligatory, when preferred, and when dispreferred (and even when impossible, given that some lexical items obligatorily have uncontracted *öyö*). In other instances absence of alternation provides no evidence for or against the derivation of *e* from */öyö/*. The morphologically justifiable cases of *öyö*-contraction indicate that we do need such a rule with idiosyncratic conditioning factors, so there is nothing to prevent us from extending it to cases where there is no such morphological evidence, again with idiosyncratic conditioning.

Turning to the possible derivation of *o* from */öwö/*, it should first be noted that there are far fewer instances of transparent morphological justification for this analysis, simply because there is no morpheme with *w* parallel to the negative allomorph *-öy*. We do, however find occasional clear examples of *o* deriving from

öwö in alternative pronunciations, as in *ölöw-öl-öŋ* 'dawn-NEGATIVE-PAST:3SINGULAR', i.e. 'it did not dawn', which may be pronounced either as given or as *ölölöŋ*. There are, however, a large number of words with *öwö* and no contracted variant, e.g. *yöwö* 'yes', and a large number of words with *o* and no uncontracted variant, e.g. *nobl* 'husband's brother; (male ego's) brother's wife', *mo* 'lower part, under'. There is even a particle parallel in behaviour to quotative *e*, namely exclamatory *ö*, before which *o* simply drops, rather than combining with a putative underlying /*öwö*/ to give **awö*, e.g. *nöb o* 'oh man!' (cf. *nöbö* 'man'), not **nöbawö*. Thus, the situation with respect to *o* from *öwö* is essentially the same as that with respect to *e* from *öyö*, the main difference being that there are even fewer relevant forms.

To conclude this section, we observe that some instances of *e* and *o* must derive from *öyö* and *öwö* respectively, though there are problems in generalising this analysis to all instances of *e* and *o*. Conceivably, *e* and *o* are phones in the process of developing into separate phonemes, or having recently developed as such, a development facilitated by the existence of loans from Kobon and of particles like quotative *e* and exclamatory *o*, perhaps originally interjections outside the regular phonological system.

5. *a*

The vowel *a* is very frequent in Haruai. In a few instances, however, there is clear morphological evidence that phonetic *a* should be derived from the sequence /*öö*/. These instances involve for instance verb forms ending in *-ö* (the suffix of the second person singular) before a further suffix or particle beginning with *ö*. One such suffix is the evidential suffix *-öd* 'I see. . .; so. . .; . . . so it appears', which appears unmodified in, for example, the third person singular *pal-öd* 'hit(-PRESENT-3SINGULAR)-EVIDENTIAL', i.e. 'he hit, so it appears'. In the second person singular, the corresponding form is phonetically [paylad], morphologically {*pal-l-ö-öd*} 'hit-PRESENT-2SINGULAR-EVIDENTIAL'. Another suffix behaving similarly is alternative *-ön*, which is attached to the first part of an alternative question, as in *n pay-l-ön pal-öy* 'I hit-PRESENT(-1SINGULAR)-ALTERNATIVE hit-NEGATIVE(-PRESENT-1SINGULAR)', i.e. 'do I hit or do I not hit?' in comparison with *nagö* [paylan] (<{*pal-l-ö-ön*}) *pal-öy-ö* 'you hit-PRESENT-2SINGULAR-ALTERNATIVE hit-NEGATIVE(-PRESENT)-2SINGULAR', i.e. 'do you hit or do you not hit?'. Since Haruai does not tolerate the phonetic sequence *öö* (or, indeed, any phonetic vowel sequences), *öö*-coalescence would be an obligatory rule.

Again, the question arises whether all instances of *a* should be derived in this way. Since Haruai disallows the phonetic *öö* sequence, there can be no counterexamples to such an analysis, though equally in the vast majority of instances there will be no morphological or other evidence in favour of deriving *a* from /*öö*/. We can therefore only say that this is a possible analysis. One small piece of external evidence in its favour is a tendency for loan words to have the vowel *a* replaced by *ö* in Haruai, suggesting that *ö* is in some sense more basic than *a*; the example of Haruai *höpeb*, probably from Kobon *hapeb* 'wild', was cited in section 4.

6. *ö*

In the above, we have shown that the putative vowels *i*, *ɨ*, and *u* should be removed from the phoneme inventory of Haruai, and that moreover it is possible, though with some reservations, to remove the vowels *e*, *o*, *a* as well. If we accept the fullest version of this analysis, Haruai would be left with just one vowel phoneme, *ö*, making it an instantiation of almost the most extreme version of a Sepik-Ramu vowel system. The most extreme type, of course, would have no phonemic vowels. In earlier literature on alleged one-vowel languages, centring primarily on the North-West Caucasian languages,⁸ the claim has been advanced that it is impossible for a language to have only a single vowel phoneme: Since the essence of the phoneme is contrast, how can one have a vowel that does not contrast with anything else? The answer to this question is that although Haruai *ö* on this analysis, does not contrast with any other vowel, it does contrast with zero, i.e. the absence of any vowel (or other segment). This can be seen clearly in minimal pairs like *yw* 'knife' versus *ywö* 'meat'. Thus Haruai does have (at least) one vowel.

7. CONCLUSION

Haruai can be analysed, with some degree of plausibility, as having only a single vowel, the mid central vowel *ö*. If the problems discussed in sections 4 and 5 are considered sufficient not to deny phonemic status either to *e* and *o* or to *a* or to all of these, then the vowel system will be *ö-e-o*, or *ö-e-o-a*, in any event a vowel system lacking high vowels. The phonetic high vowels *i*, *u*, *ɨ* are not part of the phonemic system of Haruai, but arise solely as the result of allophonic rules assigning syllabicity to basically consonantal phonemes; analyses attributing high vowel phonemes to Haruai either miss generalisations or require additional stipulations that serve only to complicate the analysis.

NOTES

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2. Palatalisation is indicated here by a tilde, based on the diacritic used in representing the palatal nasal /ɲ/.
3. Transcriptions in square brackets are narrower than phonemic, but only to the extent that is necessary for the point under discussion, so that, for instance, the allophonic prenasalisation of the *b* of *y^b* is not here indicated. Word-initial high front unrounded and high back rounded vocoids before a true consonant are more accurately described as a sequence of semivowel and vowel, i.e. [yɪb], but a simplified transcription is used here, indicating merely that the overall realisation is syllabic.
4. As noted in section 5, in the sequence *ō+ ö*, the first *ö* is not dropped, rather the two *ös* combine to give *a*, so that *dy-n-ö* plus the evidential suffix *-öd* gives *dynad*. I assume that in such cases the rule of *ö*-deletion is bled by the preceding application of *öö*-coalescence. In any event, *ö* cannot remain before a further *ö*, in the way it remains before a consonant.
5. There are no verb stems ending in *y*, but this follows from the constraint against a verb stem ending in a palatal(ised) segment.
6. Just as /y/ and /w/ require further specification of their syllabic allophones (*i* and *u*, rather than *yⁱ* and *wⁱ*), so too /h/ has an exceptional syllabic realisation, namely *h^ö* rather than **hⁱ* (which latter never occurs). This leads to the neutralisation of the opposition between underlying /h/ and /h^ö/, so that while *dy-n* 'go-FUTURE(-1 SINGULAR)' and *dy-ön* 'go-FUTURE(-3 SINGULAR)' are phonetically distinct through different allomorphs of the future tense suffix, the corresponding forms of *h* 'come' are homophonous, i.e. morphological *h-n* and *h-ön* are both pronounced [h^{ön}].
7. The morphological structure of *dw-öy-ö* requires some further comment. The present tense suffix in the first and second person singular is *-l*, which (in most instances, optionally) palatalises a preceding *l*, e.g. *pay-l-ö* or *pal-l-ö* 'hit-PRESENT-2SINGULAR' 'you hit'. In the negative, the present tense suffix *-l* obligatorily palatalises the *l* of the negative suffix *-öl* and is itself (quite exceptionally) deleted, although its trace is visible in the palatalisation of the *l* of the negative suffix. The morphological structure of *pal-öy-ö* is thus {pal+öl+l+ö} 'hit-NEGATIVE-PRESENT-2SINGULAR'.
8. For a survey of the history of this controversy, see Hewitt (1981:205-7).

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MARKED PRONOUNS AND VERBS FOR MARKED SOCIAL RELATIONSHIPS IN A CHADIC AND A PAPUAN LANGUAGE

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INTRODUCTION¹

Many of the studies of marked usage of pronominal and verbal categories corresponding to a marked social relationship are influenced by the study by Brown and Gilman (1960) of pronominal usage in a number of languages, and in particular English, French, Italian, Spanish, and German. The authors point out the close association between historically observable T/V alternation systems² and two 'semantics' or abstract social

PART V

PREHISTORY AND ORAL HISTORY



HUNTING FOR WALLABIES: THE IMPORTANCE OF *MACROPUS AGILIS* AS A TRADITIONAL FOOD RESOURCE IN THE PORT MORESBY HINTERLAND

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INTRODUCTION

In 1969 Ralph Bulmer, as head of the Department of Anthropology and Sociology in the University of Papua New Guinea, was instrumental in the University's purchase of Motupore Island, some 15 km east of Port Moresby. Motupore has been developed as a marine biology station, but has also continued, since the early 1970's, to be the focus of archaeological research, since it is the site of a large late prehistoric village which has remained a central focus for reconstructing Port Moresby's prehistory. Research there led me into a series of interpretative disputes with colleagues Susan Bulmer and Nigel Oram (Allen 1977, 1984; S. Bulmer 1978, 1979; Oram 1977) on the role and history of the Western Motu's famous trading expedition, the *hiri*. Ralph Bulmer, who had a keen and continuing interest in the integration of archaeology, ethnography and the natural sciences (e.g. Bulmer and Bulmer 1964; R. Bulmer 1971, 1984) presided over a number of these disputes with good humoured objectivity. It is in respect of this interest that this note is offered.

While disputing the dynamics of Port Moresby prehistory, Oram (1977:80-7), S. Bulmer (1979:5-11) and I (Allen 1977:447) accepted a long-term assessment of the Port Moresby environment: in 1877 Turner (1877:481) had pronounced the land in the neighbourhood of Port Moresby to be 'barren and unproductive', a view often since repeated. Prehistorians of the Port Moresby region, myself included, have found it impossible not to appeal to environmental determinants in reconstructing the local prehistory. Thus both Oram and I see the *hiri* as a mechanism for alleviating the problem of low levels of available local foods during the lean season (late wet - early dry) around Port Moresby, but disagree on the imperatives which gave rise to this situation.

In an important paper contributing to the wider debate, Vasey (1982) attempted to judge Oram's and my positions by questioning the basic premise, that local production in the Port Moresby region was unable to support a notional population of 2,000 people throughout each year because of the infertility of the soils and the low productivity of the natural environment.

In the course of this investigation Vasey looked at the contribution of hunted wallaby meat to local food consumption. In reconstructing the annual food round it had been previously widely accepted that wallabies constituted an important seasonal protein source in the late dry season (roughly June-October) at a time when local garden produce was largely consumed and when many Western Motu men were involved in the practical and ceremonial preparations for the *hiri*. Prominence in these reconstructions is given to the notion of Koita and Koiari men, inland from the Western Motu, hunting wallabies and trading their smoked carcasses into the coastal villages in the Port Moresby region.

Taking a contrary line, Vasey (1982:139) argued that the value of hunting wallabies in terms of potential local food productivity had been overestimated; he considered that hunting should be categorised as a 'high risk strategy' - one in which the dependability of gaining an anticipated return for effort is low. It is my impression that here Vasey was embracing an agricultural prejudice that sees hunting and gathering communities being at the whim of nature. This leads into a different argument which will not be pursued here; instead this note examines Vasey's arguments concerning the contribution of wallaby hunting to local food production.

EXAMINING THE VASEY HYPOTHESIS

Vasey pointed out that in a survey on Goodenough Island, Liem (1977) had demonstrated that *Macropus agilis* is carried at about one animal per ha on *T. australis* grasslands. On the basis of a similar carrying capacity for Port Moresby, which Vasey considered generous, since the Goodenough dry season is less severe than that around Port Moresby, and allowing a sustained annual yield of one quarter of the carrying capacity, Vasey calculated that wallaby yields would be 1.5 animals per person per annum for his notional population of 2000. In addition, Vasey questioned one account which suggested that a single hunt yielded between 500 and 1000 animals, concluding that even if taken on face value 'it would best be interpreted as evidence of boom and bust cycles in hunting' (1982:139).

While the putative consumption rate calculated by Vasey appears accurate within the parameters and premises he employs, the figure of 1.5 animals per person per annum can be questioned as a measure of

prehistoric consumption of wallaby on several bases. Firstly, since no carrying capacity studies of *Macropus agilis* in the Port Moresby region have been undertaken, it is not possible to judge how accurately Goodenough Island figures can be adopted for Port Moresby, even allowing for a longer dry season there. Secondly, savannah, and presumably the distribution of wallabies, extends well beyond the artificial research area used by Vasey. I calculate that between Boera and Bootless Bay and 15 km inland along this stretch of coast that there are between 30,000 and 40,000 ha of savannah and grassland, so that the 12,000 ha figure used by Vasey should not be taken to indicate a real distribution range for these animals, even allowing the carrying capacity figure of one animal per ha. Thirdly, while such an area is within easy trading distance as well as local capture distance of all the people living within its boundaries, we have no idea of human population sizes within it through time nor any real understanding of their food extractive strategies. On the one hand Vasey's putative 2000 people are estimated to occupy and exploit an area much smaller than the one considered here and thus we might assume a larger population in the larger area. On the other hand we could assume that the ethnographic population figures used as a basis by Vasey, while accurate, represent a period of increased population, and that for significant periods of the past millennium the larger area contained fewer people. Finally, wallaby hunting was a predominantly seasonal activity which supplied meat at a particular time of the year, so that per annum consumption rates have little meaning. What is more important is that, taken towards the end of the dry season, these animals provided a particular seasonal food.

ETHNOGRAPHIC EVIDENCE

Vasey also questions the accuracy of ethnographic descriptions of wallaby hunting. It is obvious that references made in passing to wallaby hunting by missionaries and administrators at the end of the nineteenth century may not be accurate in detail, nor will they necessarily provide a satisfactory alternative to Vasey's attempt to estimate productivity of this food source. They do however provide a qualitatively different conclusion to that reached by Vasey, and at the same time add further understanding of the ways in which wallaby meat contributed to the diet.

Utilising the northwest-southeast trending valley floors, the consistent southeast trade wind and the usually extremely dry grass cover towards the end of the dry season, the principal hunting technique was to fire a valley and drive the game in it northwestwards towards a barrier of nets about 1.5 m high, where the animals would be despatched with spears or captured in smaller handnets (Turner 1877:487; Lawes 1880:610; Romilly 1893:224-5). Such hunts were obviously communal, involving according to Lawes (1880:610) 'all the men and boys of the whole village'; 'hundreds of natives' by Romilly's (1893:325) account - quite likely to be an exaggeration but still indicating a lot of people; 'a great many natives' (Chalmers and Gill 1885:36); and the amalgamation, at least in Koiari territory, of groups of men from different hamlets: 'A fortnight ago there was a great wallaby hunt down at Moumiri, and natives from all the districts round were present' (Chalmers and Gill 1885:121).

The method of fire-driving, requiring that the countryside be dry, at least partially dictated a particular time of year as a hunting season, since this technique ensured much higher returns for effort expended than opportunistic spear hunting at other times. Given this, it seems reasonable to question Vasey's categorisation of wallaby hunting as a high risk strategy since extreme dry season dryness is one of the more reliable aspects of the annual environmental scene in the region.

S. Bulmer (1978:43) suggests June and July as the hunting season. Turner (1877:487) says the season begins at the same time as the hunting season 'at home', presumably meaning late summer/early autumn in Great Britain. The hunt witnessed by Romilly (1893:321-9) took place a few days before June 20 1878 (1893:311). Better data are supplied, *inter alia*, by Chalmers and Gill. Late in October 1877, possibly the 29th, a large wallaby hunting party departed from Hanuabada, returning the same afternoon after a 'successful day's hunting' (Chalmers and Gill 1885:36). In 1879 Chalmers undertook an inland expedition visiting many villages and exploring the mountainous country along the course of and between the Goldie and Laloki Rivers, reaching villages directly inland from the eastern end of Bootless Bay. This trip started on 15 July and concluded on 26 September. Despite detailed descriptions of the people, their activities and the food locally supplied to himself and his party, Chalmers makes no mention of wallaby hunting prior to 18 August, although he talks of pig and cassowary hunts and an unidentified hunt on 9 August, involving everyone in the particular village (Chalmers and Gill 1885:101, 106). On the 18th, Chalmers notes that he cannot get carriers because all the men are off hunting wallabies (1885:108). They return on 21st with 'large supplies' (1885:110). The Moumiri hunt, referred to above, had taken place around the 15th (1885:121). Chalmers further reports wallaby hunting on the 31st (1885:121), eats smoked wallaby on 6 September in a village behind Bootless Bay (1885:127) and mentions wallaby hunting which had occurred again near Moumiri prior to 24 September but after 29 August. While the earlier absence of references to wallaby hunting does not prove that these animals were not being hunted prior to mid-August in 1879, they were certainly taken in late August and probably throughout September.

In summarising these references for the three-year period 1877-1879 it can be seen that wallaby hunting continued to the end of October in 1877, recommenced before June 20 in 1878 and occurred from at least mid-August to late September in 1879. It should not be concluded from this, however, that a normal hunting season continued for up to five months. More likely, climatic factors, and particularly conditions of relative dryness, dictated when the season might commence in any particular year.

While the Western Motu at contact participated in wallaby hunting, it was principally an occupation of the Koita and Koiari. After capture the carcasses were smoked, presumably to aid short term preservation and 'large quantities' were bartered by the Koita and Koiari in the coastal Motu villages for other items (Lawes 1878:373).

Without being able to estimate wallaby carrying capacities on the savannah and grasslands around Port Moresby and on the inland plains, Europeans moving through the area at the end of the nineteenth century frequently commented on their numbers. Thus, for example, Chalmers in late October 1877 spoke of 'great herds' between Port Moresby and the Laloki River (Chalmers and Gill 1885:32), the annual report for 1885 also commented on the large numbers of wallabies (BNGAR 1886:20), Turner (1877:487) noted that they 'abound through the bush' and Romilly's account, discussed below, also suggests large numbers.

Quantitative accounts of the return on hunts in the ethnographic accounts are limited to the single testimony of Romilly. In reviewing this Vasey notes that:

some topographic features are exaggerated, and the author states that on the day of the hunt, 23 June, it had not rained in six months, and yet grass was six to eight feet tall, which would indicate a long previous wet season. If accurate, the wet and dry season of the previous year would be in reverse order from normal. Even if the account is taken at face value it would best be interpreted as evidence of boom and bust cycles in hunting (1982:139).

Leaving aside the question of whether or not the seasons had been reversed, which is not obvious to me from the stated facts, it is clear from reading Romilly's letters that, in general, he tends towards exaggeration and imprecision in quantitative statements - thus, for example, the 'six months' might only have been four, which would imply a more normal season. The accuracy of any of Romilly's statements may only be judged from the nuances of his prose and it is worthwhile, therefore, to reproduce verbatim extracts of his account.

Romilly arrived at the hunt some time after its commencement. 'A huge tract of country was on fire, quite fifteen miles square as we estimated' (1893:323). This probably means 15 square miles for shortly after he writes 'the ground was certainly well chosen, for in the existing state of the wind the natives could fire the grass four or five miles away from the hill, and a wall of fire, three miles in width, would then come sweeping along. . . and in that long dry grass seemed to take huge strides of fifty yards at a time' (1893:324) driving the game towards 'a strong barricade of nets, which stretched for some hundreds of yards across the narrow pass, into which all game which had not escaped fire and spears would of necessity be driven' (1893:325). 'The grass averaged about five feet in height, but was in many places as high as eight or ten feet, and it was as dry as tinder, for, for six months no single drop of rain had fallen' (1893:324).

Finally the hunt was over:

There was at no time a dense rush of kangaroos such as one sees sometimes in Australia, but for all that a heavy slaughter which we could not see was being kept up all along the line of fire. Nor could we tell at the conclusion of the day's sport what the total bag was, for each man made off with the portion that had fallen to his share as fast as his legs could take him. I have no doubt, to put it at the lowest estimate, that 500 kangaroos were killed, and perhaps fifteen or twenty pigs. Perhaps the figures might have been double that amount (1893:327).

Whatever the real figure, the general order of magnitude of the capture from this single hunt, together with the general ethnographic observations which suggest that such a hunt was not disproportionately large, allow some reconstructed figures to be generated. Ten such hunts in a three month season, which would seem at a guess to be reasonable, would yield between 5,000 and 10,000 wallabies. If each averaged 10 kg of edible meat this would result in between 0.25 and 0.5 kg of meat for 2,000 people every day for three months.

The edible meat figure is based on the following estimates. *Macropus agilis* attains weights up to 16 kg for the female and 26 kg for the male (Merchant 1976:93). Sexual maturity is attained at ca. 12 months after a pouch life of ca. 7 months and the normal life expectancy is 10-12 years (Ross 1977:7). If the animals are at adult size for three quarters of their natural life span and allowing for sexual dimorphism, an average capture weight of 15 kg can be estimated. Dwyer (1980) in a study of edible to waste ratios on 27 species of highland New Guinea mammals found that the edible parts of these species ranged between 50% and 82% (1980:111), with the figures for the Lesser Forest Wallaby, *Dorcopsulus vanheurni*, being 69% and the Ornate Tree Kangaroo, *Dendrolagus goodfellowi*, being 75%. Dwyer (1980:110) considered that these figures might be

fractionally underestimated. While these figures may not be directly comparable to *M. agilis*, 66% of 15 kg provides an edible meat estimate of ca. 10 kg per animal.

A second independent set of hypothetical figures for seasonal capture rates can be calculated from Romilly's figures. Fifteen square miles is approximately 3800 ha. Thus, the capture rate for Romilly's hunt was between one animal per 3.8 ha and one animal per 7.6 ha. If the full extent of the grassland, estimated at 35,000 ha, was exploited at this rate it would yield between 4,600 and 9,200 wallabies, a figure similar to that generated above. As well, it is interesting that the capture rate of one animal per 3.8-7.6 ha generated by Romilly's figures encompasses the capture rate employed by Vasey of one animal per 4 ha.

ARCHAEOLOGICAL EVIDENCE

Ultimately, all such estimates can only present us with probabilities. In the absence of any written record, the archaeological record provides the strongest direct evidence on the question of the contributions of wallabies to the diet. In my excavations on Motupore Island I recovered a total of 38,097 non-human bones representing mainly or totally the remains of human meals. These weighed 63.7 kg. Of these almost exactly half were too fragmented to be identified. Of the remainder, the majority, as might be expected on an island, derived from marine animals - mainly fish, turtle and dugong. Of the identified bone these accounted for 66.4% by number and 63.1% by weight. Of the terrestrial component, which included the domesticates, pig and dog, the 5,977 wallaby bones represented 93.2% by number and 89% by weight. In the total identifiable sample wallaby constituted 31.3% and 32.9% by number and weight respectively. These are minimum representations since most of the unidentified bones in the faunal assemblage are long bone fragments which could only logically derive from wallaby, pig, dog or cassowary. Given the tiny representations of the latter three animals and given that wallaby long bones are under-represented by both number and weight, compared to other body parts (Allen and Guy 1984:43) it follows that there is significantly more wallaby bone in the assemblage than has been identified.

Examination of the seasonal stress patterns in the teeth of these animals suggest that they were mainly killed at the end of the dry season, which conforms to the ethnographic model elaborated above. As well, reconstructions of the age distributions of the wallabies in the site suggest that neither very young nor very old animals occur. The absence of young animals is thought to mean that these were uneconomical to transfer to Motupore Island, but this reasoning would not explain the absence of the older animals. What is suggested in this case is either deliberate culling of a younger range of the population or that the population from which the sample has been drawn was under heavy predation. If the Motupore evidence has resulted from behaviour similar to the ethnographic descriptions, then the latter would likely be the case, since fire-driving animals will presumably result in indiscriminate rather than selective capture.

CONCLUSION

These archaeological data do not tell us what either the per capita annual consumption of wallaby meat may have been, nor, of course, what relative contribution wallaby meat may have made to a total diet which included plant foods unaccounted for in data such as these. They do suggest, however, that wallabies provided a principal source of red meat in the region over a considerably longer period than that encompassed by the ethnography. Collectively these data call into question whether wallaby hunting around Port Moresby was a high risk strategy (especially in comparison to other subsistence strategies used locally). They suggest that seasonally, and in most dry seasons, wallaby meat made a significant contribution to local diet.

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MANUS, MORTARS AND THE KAVA CONCOCTION

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INTRODUCTION

This paper addresses yet again the question of the function and origin of the finely-fashioned stone mortars and pestles from New Guinea which Ralph Bulmer and others enquired into and published on in the 1960's. I have set out to suggest that some connection exists between the development of the widespread stone mortars and their use in the early preparation, selection and propagation of the important Oceanic plant, kava. The suggestion is based on the possible role of mortars in the preparation of distasteful concoctions, and their apparent distributional overlap with wild and cultivated forms of kava, *Piper wichmannii* and *Piper methysticum*.

ASCRPTIONS OF FUNCTION

During 1963-64 Ralph Bulmer spent six months in the Highlands of eastern New Guinea, working with the Kalam people of the Kaironk Valley and, among other things, recording local perceptions of the natural environment. Stone pestles and mortars were notable artefacts of the region, along with their occurrence in a broad area of the Highlands and the Bismarck Archipelago. Local informants had no consistent explanation for the origin, manufacture or use of these sometimes elaborately fashioned stone objects. A search for a functional and historical explanation of the mortars and pestles, and other artefacts formed by pecking, grinding and polishing, such as stone club heads, has exercised many commentators on the material culture of New Guinea. Some suggestions about the age and possible uses of stone mortars were made by Bulmer (1964) who favoured the option of seed and nut grinding, especially of the edible nuts of *Castanopsis acuminatissima* and other rain forest tree species.

In support of the nut-grinding application for mortars and pestles Bulmer pointed to a concordance between the altitudinal limits of *Castanopsis* and the reported locations of mortars. *Castanopsis* requires humid conditions but may occur from the lower montane zone to sea level in some favourable areas (Paijmans 1976:65).

PART VI

ANTHROPOLOGY AND THE MODERN WORLD



**SOVEREIGNTY vs RANGATIRATANGA:
THE TREATY OF WAITANGI 1840 AND THE
NEW ZEALAND MAORI COUNCIL'S KAUPAPA 1983**

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Each of the two parties to the Treaty¹ invested it with expectations about the exercise of power. The Maori expected his 'Rangatiratanga' to be protected; the Crown expected to gain sovereignty over New Zealand. The purpose of the Treaty, therefore, was to secure an exchange of sovereignty for protection of rangatiratanga. Such was the unequivocal view of the Treaty of Waitangi held by the New Zealand Maori Council in 1983 (NZMC 1983:4),² and at the heart of the matter for them was the Crown's guarantee of protection of rangatiratanga (NZMC 1983:5). It was this guarantee, first given to the Maori people in 1840 and reiterated many times thereafter, that shaped the Council's Kaupapa (submission) to government, not for a ratification of the Treaty in a literal sense, but for statutory protection wherever Maori interests required it.

We shall proceed by considering the subject of the Crown's guarantee given in 1840 and what was in turn to be given by the Maori to secure that guarantee. For the Maori of 1840 the agreement with the Crown was moral rather than legal in character, with political costs and benefits circumscribed by tribal and sub-tribal horizons that until that time had suffered the intrusion of little more than an occasional missionary or trader. Maori expectations of the Treaty, therefore, were dependent neither upon legal experience nor legal advice, but simply upon trust of missionary interpretation and counsel and upon their own individual and tribal ambitions. Thus an understanding both of the Maori signatories' faith in the integrity of the Crown and its missionary advocates and of the meaning of rangatiratanga is crucial to an understanding of the significance of the Treaty to the Maori people. And it is equally crucial to an understanding of the Maori Council's view that it should justify its submissions to government at the close of the 20th century by reference to this self-same Treaty.

However let us begin with the 'sovereignty' factor in the Treaty before turning to that of 'rangatiratanga'. The conventional view and one adopted by the New Zealand Maori Council was that the Crown did indeed seek sovereignty over New Zealand, obtained it formally by means of the Treaty, and with it proceeded to establish constitutional government. Yet for almost the whole of the 70-year span following Captain Cook's 'discovery' of New Zealand in 1769, sovereignty was anathema to the British government. For instance, in 1817, the first of several statutory declarations issued from the British Parliament asserting that New Zealand lay outside its jurisdiction (Wards 1968:2). Nevertheless with a base already established in New South Wales, Britain continued to trade, to export, and to evangelise her way around the South Pacific, including the North Island of New Zealand, so lending some antipodean substance to the doctrine of the 'imperialism of free trade' (Boyd 1972:4f). And this seems to have been the essence of it: the British government was willing to allow, perhaps even encourage, economic and ideological expansion, providing it did not entail formal territorial annexation, a budget, or a military back-up. Given the rate of expansion of the British Empire at the time and the consequent drain on resources, such an informal approach to conquest made sense. But to the entrepreneurs, the traders and the missionaries at the 'frontier' in New Zealand there was no lasting profit to be made without security and security by the 1830s was at a premium, with internecine tribal warfare made more deadly by the musket, a lawlessness endemic among much of the small scattered European population, and a provocative foreign presence being provided by the French (supported by the Church of Rome), and by the U.S.A. (Wards 1968, Chapter 1; Ward 1978, Chapters 2 and 3; also Miller 1958, Oliver 1960, and Sinclair 1984).

Mounting trader and missionary agitation and the Maori people's apparent lack of political institutions that could be adapted to cope with settlement and development finally forced the Colonial Office to take heed of proposals and reports channelled through New South Wales, particularly the proposal to appoint a British Resident to New Zealand. An appointment was made in 1833, but in the event it bordered on the farcical, for the Resident, James Busby, arrived at his post in the Bay of Islands totally devoid of effective power and authority. Yet for the subject under review the appointment was not without significance. First, it marked an unequivocal step towards the acquisition of British sovereignty. And second, Busby's immediate organising of a 'Confederation of Chiefs' conjured up an image of an indigenous sovereign authority that doubtless would have to be consulted in any future negotiations with the Crown; an authority, indeed, that might well prove to be anxious for negotiation, given the continued decimation of the Maori people by war and disease at the time (Ward 1978:24; Wards 1968:8-14).

Nevertheless the political situation on the ground was unchanged: the security desired by missionary, trader, and many of the chiefs was neither provided in substance nor pledged in principle. Pressure on the Colonial Office increased accordingly. Busby sought powers to make the confederated chiefs an effective government; the missionaries cited Maori Christian communities who were willing to submit to British laws; Captain Hobson RN, sent on a fact-finding mission by the Government of New South Wales to the Bay of Islands, recommended the assumption of sovereignty over the whole of New Zealand; and not least, there were reports that land speculators (notably the New Zealand Company) were buying up large tracts in New Zealand and establishing their own brand of sovereignty. Thus in 1839, in a muddle of political expediency, Victorian compromise and an ignorance of the evolving political and cultural relations in New Zealand, the Colonial Office finally concluded that it could no longer avoid grasping the nettle and that it should forthwith '... obtain sovereignty, incur no avoidable expense, see what happens' (Wards 1968:28).

By such a strategy it hoped to control the anarchy that threatened its influence. Its tactics were to offer a treaty to those now deemed to hold sovereign power in the country, namely the chiefs (whether or not they belonged to the Confederation), whereby they would cede this power to the Queen in exchange for a limited autonomy and sundry rights and privileges of British subjects. The man appointed to negotiate the sovereignty was the author of the report on conditions obtaining in New Zealand, Captain Hobson. His instructions from the Colonial Office and their transformation into a three-article treaty in English and in Maori have become the subjects of endless analysis and debate.³ Often, comment has focused on the Crown's guarantee to protect the Maori in the ownership of his land – comment made more pertinent by the evident loss of this ownership, which began almost as soon as the treaty had been signed. But this notwithstanding, a prior consideration, i.e. what the Maori was to give the Crown before the latter would give anything, was sovereignty.

Views about sovereignty as held by some in London and as expressed by others 12,000 miles away in New Zealand (and six months or more later) were never likely to be identical. Even less likely was a congruence between the expectations of policy-makers in the Colonial Office on the one hand and those at the interface between the two ethnic groups in, for instance, the Bay of Islands, or Port Nicholson (Wellington), on the other. To complicate matters further the policy-makers had no personal knowledge of New Zealand and the frontier conditions obtaining there, let alone any understanding of the social institutions and culture of the Maori people. Hence what they designed in pursuit of the goal of sovereignty drew its inspiration largely from a general pool of colonial experience and from whatever social philosophy happened to be in vogue at the time. Yet the policy at this pre-Treaty stage did express a humanitarian concern for the Maori. Sovereignty by conquest was never considered an option. Even peaceful annexation was not to serve the Maori people ill – for instance, that they should not be allowed to suffer at the hands of the settlers, and that by becoming British subjects they were not only to receive the protection of the Crown, but also with patience and tolerance they might be weaned from their savage state so that ultimately Maori and European could settle down to a mutually profitable and enduring relationship (Ward 1978:30-4).

While therefore it was gradualist it was also openly assimilationist and ethnocentric. In particular it made no allowance for the continued exercise of Maori tribal authority in the government of the colony, although at the time the Maori would have controlled the entire country and outnumbered the European by upwards of 100 to 1 (Ward 1978:37-9).

Let us turn now to consider some of the facets of rangatiratanga. In the context of the Treaty of Waitangi there is a rangatiratanga that may be understood by reference to Maori values and institutions, and a rangatiratanga that may be understood by reference to European values and institutions, specifically, a Protestant view of Christianity on the religious plane, and the British monarchy and government on the secular.

At its most general, rangatiratanga (being derived from 'rangatira', chief) means 'evidence of breeding and greatness' (Williams 1975:323; also Introduction to Kawharu (ed.) 1989:xixf). Here, 'breeding and greatness' allude to the two main criteria for leadership: primogeniture (generally male) and proven ability. 'Evidence', for its part, turns on the concept of 'mana'. Mana is that power and authority that is endowed by the gods to human beings to enable them to achieve their potential, indeed to excel, and, where appropriate, to lead. It is in the nature of a spiritual contract mediated by the priests, chiefs and elders of a tribal group between an individual member of the group and their deities. What is looked for, then, in a rangatira is evidence of a working out of a high order of spiritually sanctioned power and authority. Primogeniture, insofar as it refers to proximity by way of a line through the ancestors to the supernatural source of such power and authority, may thus be called the prescribed factor in rangatiratanga, and ability the achieved factor. Implicit in all of this is the matter of reciprocity: between the individual and his god, and between the individual and his tribal community. Both sets of relationships are specific and closed to the community, both operated only within the laws and checks and balances of its political system. A rangatira is a trustee for his people, an entrepreneur in all their enterprises.

So much by way of a note on rangatiratanga defined in terms of Maori custom – the sort of rangatiratanga that would have been understood by the Maori of 1840. ‘Rangatiratanga’ had also, however, been used by Anglican and Wesleyan missionaries for many years prior to the signing of the Treaty of Waitangi to convey the sense of power and authority that emanated from the Christian God, e.g. ‘thy Kingdom come...’: ‘... Kia tae mai tou *Rangatiratanga*. . .’. But it was a power and authority which owed its efficacy to a system of rewards and penalties quite other than that defined by Maori custom. Granted there was a spirituality at its source, but otherwise the rangatiratanga of the Christian God was conceived on an entirely different plane, one dominated by ideas of a benevolent Supreme Being, a set of principles of morality, a promise of deliverance from sin and damnation wrought by Christ’s atonement, and so forth.

And the same kind of distinction could be drawn between a Maori chief’s rangatiratanga and the Queen’s rangatiratanga; that is, while both were similar in having spiritual and secular dimensions, the laws and sanctions upholding each were derived from different cultural bases and so operated differently. Neither the Christian God nor the Queen of England were tribal figures with powers limited to a tribal and subtribal politico-religious system. Hence while each possessed qualities of ‘breeding’ and ‘greatness’, and displayed all the attributes of mana, there was no necessary reciprocity between either of them on the one hand and an individual Maori on the other in terms of the latter’s tribal status: tribal identity and ethos were as irrelevant to the rangatiratanga of the Queen as to the rangatiratanga of the Almighty.

As a corollary, the rangatiratanga or sovereignty of the Queen, being defined simply by the criteria of her system of government, might be described as government-type rangatiratanga. Indeed in the Treaty this kind of rangatiratanga was transliterated into Maori as ‘governor-tanga’ or ‘kawana-tanga’, the machinery of government, that had little in common with the rangatiratanga of a tribal chief.⁴

Such ‘non-Maori’ use of the word rangatiratanga, then, had some logic to commend it. But, as will be shown later, it was not so much the different cultural frames of reference to which it might be attached that was problematic, as the different and quite specific expectations which it generated when incorporated into a treaty document.

To return: it had become clear that by 1839 Britain was prepared to acquire sovereignty over New Zealand in furtherance of its own vested interests. But it was equally clear that a number of Maori chiefs were also anxious for the protection of their people and their own rangatiratanga against the depredations of their neighbours. They had realised that since neither Resident Busby nor the various tribes themselves had been able to create an effective centralised government, they would have to attract and hold a power superior to that of any of their number before there could be some prospect of law and order.

Probably the first overture for such a purpose had been the appeal made in the name of the Confederation of Chiefs in 1835 for the Crown’s protection, an appeal which had elicited a clear, if guarded, response: ‘... (His Majesty) will not fail to avail Himself of every Opportunity of showing His Goodwill, and of affording to those Chiefs such Support and Protection as may be consistent with a due Regard to the just Rights of others. . .’ (Ross 1972:130, fn 10; Elder 1932:511f – fn re Declaration of Independence of New Zealand; Wards 1968:14).

And so there was some justification for the Colonial Office assuming four years later in 1839 that a formal offer of protection would still be well received by tribal chiefs and that probably some would be willing to give the Crown the right to exercise it. In short, it would appear – as it did to the New Zealand Maori Council in 1983 – that by 1839 the Crown had come round to the position not only of seeking sovereignty for its own imperial purposes, but also of appreciating the need to satisfy the desire for peace and protection on the part of those from whom the sovereignty was to be acquired. Hence the beguiling prospect emerged that New Zealand might be had for the mere asking.

Circumstances surrounding the drafting of the Treaty are as enigmatic as its meaning. It is not even possible to pin responsibility on a single author. James Busby, Capt. Hobson and J.S. Freeman (the latter’s secretary) have all been named for their part in the preparation of ‘notes’ for a treaty – on the basis of some general instructions given Hobson by the Colonial Office in August 1839. What is clear from the historical record, however, is that these notes were translated into Maori solely by the Rev. Henry Williams (head of the Anglican Church Mission in the Bay of Islands) on 4 February 1840, and that it is this translation, eventually signed by some 500 Maori chiefs (beginning at Waitangi, Bay of Islands, on 6 February 1840), which should properly be regarded as the Treaty of Waitangi (Ross 1972:132ff).

Although the English and Maori versions of the Treaty are basically similar, one is certainly not an accurate translation of the other. That aside, the major problem arising in the first article turns on the issue of sovereignty, a system of power and authority (as would have been intended by the Colonial Office) that was wholly beyond the Maori experience, a network of institutions ultimately to comprise a legislature, judiciary and executive, all the paraphernalia for the governing of a Crown Colony.

The Maori people’s view, on the other hand, could only have been framed in terms of their own culture; in other words, what the chiefs imagined they were ceding was that part of their mana and rangatiratanga that

hitherto had enabled them to make war, exact retribution, consume or enslave their vanquished enemies and generally exercise power over life and death. It is totally against the run of evidence to imagine that they would have wittingly divested themselves of all of their spiritually sanctioned powers – most of which powers, indeed, they wanted protected. They would have believed they were retaining their rangatiratanga intact apart from a licence to kill or inflict material hurt on others, retaining all of their customary rights and duties as trustees for their tribal groups. A counterpoint to this is the fact that many of those who opposed the Treaty did so precisely because they took the view that they had no need of the Crown's protection of their rangatiratanga, or else that protection would compromise it. Thus at Waitangi, Tareha of Ngati Rehia said: 'No Governor for me – for us Native men. We, we only are the chiefs, rulers.' (Colenso 1890:24). And Te Kemara (to Hobson): 'If thou stayest as Governor, then, perhaps, Te Kemara will be judged and condemned.' (Colenso 1890:17).

Consequently the use of 'rangatiratanga' or 'mana' as the subject of cession in the first article would have been quite inaccurate – and fatal for the obtaining of signatures unless qualified in such a way as to indicate what aspects of rangatiratanga and mana were *not* being ceded. Williams may or may not have appreciated this problem and even if he had might have been greatly exercised to know how to solve it. The fact is that he chose not to use either rangatiratanga or mana, but kawanatanga, keeping rangatiratanga for Article 2 to indicate the chiefly powers that *were* to be retained, guaranteed, and protected (Article 3) by the Crown.

It is therefore somewhat misleading to suggest that mana or rangatiratanga should have been used to indicate what the Maori was ceding to the Crown (Ross 1972:141).⁵ At all events, as far as the New Zealand Maori Council is concerned it is essential not to lose sight of the quid pro quo in the Treaty and that the collective surrender to the Crown of the power to govern was made primarily in return for the Crown's protection of each chief's authority within his tribal domain (the further 'gain' in Article 3 of imparting '... all the Rights and Privileges of British subjects' being without any real significance to the Maori at the time).⁶

It might have been regarded as self-evident that kawanatanga (governor-tanga) would mean some superior authority deriving from the Queen; and to the Christian converts there was the familiar idea of 'governance'. Little wonder, then, that Williams should find it conveniently different from the Maori 'rangatiratanga' or 'mana'. Clumsy translation or not, Maori acceptance of the first article in the Treaty gave the Crown sufficient authority to set about making and administering laws and regulations and eventually to establish constitutional government in New Zealand.

Williams's handling of the second article once more focuses on power and authority, but this time the requirement is to express it in Maori terms. Williams resorts to 'rangatiratanga'. It may be speculated that Williams's perception of rangatiratanga was limited by missionary usage (above). As likely, his perception would have been limited by the common European attitude towards individualised rights in material property.⁷ What is much more certain is his lack of appreciation of all the facets and nuances of rangatiratanga to a rangatira; and, by the same token, the meaning of 'taonga'.

We may consider the first half of Article 2 further. The crux of it is the granting of the Queen's protection to the Maori people's rangatiratanga, exercised over

- a) lands (whenua)
- b) estates (kainga)
- c) all things highly prized (taonga katoa).

As may be gathered from an earlier paragraph, rangatiratanga is idiosyncratic in aspect, a combination of spiritual, psychic and physical forces unique to every individual. The application of rangatiratanga to influence the behaviour of others, and the use made of material and non-material property, is equally idiosyncratic and beyond precise formulation. That is, it is just not possible to draw up a check list of rangatira characteristics, variously quantified and qualified: it is simply a matter determined by the interaction between an individual and his milieu.

Land has always been an arena for the exercise of rangatiratanga; but in traditional times a chief, though one of great mana and rangatiratanga, would not 'own' personal rights in land greater than those possessed by others of humble rank. His rangatiratanga would be apparent by the degree to which his community accepted his leadership in land (as in other) matters. Likewise, the non-material, such as jealously guarded esoteric knowledge, was another dimension of rangatiratanga, with proof of fitness to be a trustee of the knowledge, a prerequisite to a claim to rangatira status. Rangatiratanga, then, has to be seen to be all-embracing, where only the rangatira himself can specify the limits of relevance.

The thrust of this argument is strengthened in the Treaty text by the words 'te tino' and 'taonga katoa'. Tino, here, has the meaning of quintessential: so that apart from those powers which the chiefs surrendered to the Crown for the purpose of governing, the Treaty accepts the continuing validity of rangatiratanga in its basic manifestations. It also acknowledges the scope of rangatiratanga by reference to its indispensable condition, namely land (and estates), and also, by the use of 'taonga katoa', underlines the wide-ranging extent over which rangatiratanga, with the Crown's protection, was henceforth to be maintained.⁸ Whether Williams,

Hobson and the Colonial Office were aware of it or not, such unequivocal references to rangatiratanga would have been more than enough to satisfy a Maori signatory as to the substance of the protection to be given his chiefly rights and duties, privileges and obligations; in a word, his mana. Since 1840 much has been made of the errors of omission and commission in the various versions of the Treaty, English and Maori. By and large, these are not trivial matters if considered in the light of criteria for treaty drafting. The Maori Council's view, however, is that what was intended to be agreed to by the Maori people was adequately expressed in the Treaty insofar as it hinged on an exchange of sovereignty for protection of rangatiratanga.⁹ The point at issue for the Council was that for almost a century and a half the values and concepts in the Maori version of the Treaty, i.e. *the Treaty*, even where understood, had too often been ignored by the Crown, that this had accounted for Maori anger, frustration and bitterness, but that nevertheless it was still not too late to put this to rights, still not too late for the Queen and her parliament in New Zealand to fulfil their Treaty obligation to protect the Maori in the exercise of his rangatiratanga by statutory recognition.¹⁰

A more general footnote may be added to the foregoing. Lacking a script and legal institutions, the Maori was served by the marae, not only as a forum for debate, but also for reaching decisions made binding by sanctions on trust and credibility, sanctions of ridicule or hostility or rejection. For political purposes, at least, any piece of ground would suffice as a marae, given an appropriate identification of the political groups. Discussion of the Treaty may therefore be seen in this light. Furthermore, what was said on the Maori side would not have been read (even by the literate) or shaped by direct reference to the document itself. There would simply have been verbal exchanges and impromptu speeches on general principles, of which the most relevant concerned the division of power and authority; sovereignty and rangatiratanga.¹¹

Strenuous efforts appear to have been made by Hobson to have the terms of the Treaty understood by those invited to sign it (Colenso 1890:17, 20, 33). But understanding is one thing, decisions based on it another. While understanding the key issues would have been quickly achieved if only because the issues seemed obvious, reaching a group consensus sufficient to give individual chiefs and spokesmen a mandate¹² to sign the Treaty after a few hours', rather than, say, a few weeks', consultation is strong evidence that agreement to no more than general principles could have been reached in the time allocated by Hobson and his officials. It is at this point that the moral, as distinct from the politico-legal, aspect of the Treaty becomes evident, for Hobson depended almost totally on the missionaries, not only to interpret and explain the terms of the Treaty to the chiefs in their various districts, but also to reassure them that it was in their best interests to sign it.

Customarily, the Maori has had his options shaped almost as much by the impact of the oratory and reputation of those whom he listens to on the marae as by the merits of the options themselves. This would certainly have been the case in 1840. Thus the missionaries' reputation as honest men, reasonably coherent in the Maori tongue and knowledgeable in the ways of the European, enabled them to persuade the Maori to sign the Treaty with a degree of success far beyond that which any others, particularly Hobson, could ever have achieved. This was recognised at the time and has not been forgotten since.¹³

For the New Zealand Maori Council, then, there was no better – indeed no other – basis than the Treaty of Waitangi on which to build a case for seeking statutory protection for the Maori people's interests in general, and for their remnant ancestral estates in particular. There was no doubting the existence of the Treaty, or that its existence was universally recognised. And although this existence had been bedevilled by legal debate and judicial opinion, the Maori people themselves had never doubted the force of the moral, and to some extent political, claim for recognition that the Treaty gave them. Their ancestors had felt vulnerable in 1840 and ill-equipped to cope with the forces gathering about them. And they, in their turn, felt vulnerable; though living in quite different circumstances a century and a half later, they, too, were pre-occupied with threats to identity, control over heritage, and status. Admittedly, within family and sub-tribal groups there was always the likelihood of friction arising out of jealousy and acts of deception; but the greater threat to Maori interests was still seen to lie with Crown and 'settler' – in the rhetoric of the times, in 'institutional racism'. As the Council saw it, and notwithstanding the proliferation of other ethnic groups in New Zealand in recent years, only the Maori were indigenous, only the Maori had an agreement with the Crown and thus only the Maori were in a position to call upon the Crown and her representatives to bring to fulfilment their side of the agreement they and the missionaries had so assiduously placed before the Maori people at the birth of the nation in 1840.

As might have been expected, therefore, the opening sentences of the Council's Kaupapa were forthright: 'We believe that the Treaty of Waitangi of 1840 between the Crown and the Maori people is the origin and basis of British sovereignty and constitutional government in New Zealand. We also believe that under the Treaty the Crown extends its protection over the Maori people and guarantees them their assets.' (NZMC 1983:3). In his foreword, the Council's president, Sir Graham Latimer, explained how these beliefs had inspired more than two years of discussions among district councils and experts, which in turn had resulted in the Kaupapa or guidelines for statutes determining the ownership and use of Maori land. Stress had been placed on improving flexibility in administration and on the trust concept (NZMC 1983:2). While the main theme in the Kaupapa was land, the Council made it clear that this was a temporary expedient¹⁴ and that the arguments deduced in

support of reforms in this area would be extended later into other areas; and moreover, that all legislation on Maori matters should contain a preamble acknowledging the Treaty as the foundation for that legislation (NZMC 1983:3).

Much of the first chapter of the Kaupapa is devoted to rangatiratanga and to an interpretation of it in terms of trusteeship, '... the wise administration of all the assets possessed by a group for that group's benefit'. It asserts that examples of rangatiratanga may be found in most spheres of contemporary Maori social life and that all may be affected to greater or lesser extent by statute. From the ensuing discussion the following emerges:

- i) While the concept of trustee is apparent in some legislation concerning the Maori people, e.g. the Maori Trust Boards Act 1955 and Part XXVIII of the Maori Affairs Act 1953, it often seems to be more a legal device for vesting title than one for recognising an owner's kinship responsibilities.
- ii) Taken collectively, statutes concerning the Maori people do not cohere in the Treaty of Waitangi and in the Crown's guarantee of protection of rangatiratanga.

In other words, legislation has been framed piecemeal and in European terms – generally to 'deal with' a given Maori problem where it would have been politically inexpedient not to do so; and whether or not there has been a Maori point of view on the matter. The Kaupapa puts this simply: '... justice will remain in jeopardy so long as Maori values are not included in that range of values by which the laws of this country are framed and upheld' (NZMC 1983:8).

The theme of the protection of rangatiratanga over land is developed in the second chapter initially by reference to a number of basic Maori concepts, 'tangata whenua', 'turangawaewae', and 'whanau' (NZMC 1983:10). None of these concepts appear in the Treaty and none have precise English equivalents, but all would be subsumed under any discussion of rangatiratanga. Foremost is the recognition of the relationship between land and identity – identity of the individual in the context of his tribal group, and identity of the Maori people in the multiracial context of New Zealand society. The concept is 'tangata whenua' – man or people of the soil, in a historical, proprietary, or political sense. It is significant for the present discussion in that of all the values that adhere to land the one given first priority by the Maori Council was that which reflected their people's sense of identity, a value, furthermore, they could define best in their own idiom. One of the more profound qualities of tangata whenua derives from the span of time that conceptually links the past to the future. Thus the present generation will meet their challenges inspired both by the legacies of their ancestors and by anticipating the hopes and expectations of their descendants: it is all a matter of time, of belonging, and of an acceptance of the constraint of accountability. Those who can respond to this may claim 'turangawaewae' from their kinsmen – the right to belong to a particular place and a particular community. It is in a sense, therefore, the reciprocal of 'tangata whenua'.

Past vicissitudes and ill-considered actions by former owners of tribal land have sometimes deprived their descendants of any legal entitlement in the patrimony and accordingly it has become accepted for the latter to be allowed to claim turangawaewae and tangata whenua status by descent alone. This in turn has tended to impose on those who do have legal entitlement a particular responsibility to consider ways and means of using the land for the benefit of the group, as a group.

The Kaupapa itself emphasises this, to the extent that most injunctions in the Kaupapa about use are tied to the notion of group. Thus the text includes the word 'whanau', with all the connotations, in turn, of 'whanaungatanga', the ethic of kinship. At this point the Council insists that '... Maori land ownership be viewed entirely differently from ownership as it is understood in British law' (NZMC 1983:11).

If a little strident, the insistence is nevertheless apposite. As discussed elsewhere, individualisation of tribal title which began with the Maori Land Court in 1865 and the subsequent imposition of European modes of succession and partition have resulted in a cumulative fractionation of interests in individual blocks of land and a fragmentation of interests held by successive generations of owners (cf. Kawharu 1977, chapters 6 and 7). The net effect for the Maori has been disastrous. It has jeopardised, and often prevented, effective use and administration of the land; it has encouraged owners to sell for the want of a better alternative; and it has contributed powerfully to the social and cultural disintegration of land owning groups themselves. The Council therefore believed that the law ought to enable the Maori people not only to re-group their interests for the land's more efficient use, but also, by calling attention to the tribal origins of the land, encourage the distribution of profits from its use equally among the descendants in the land rather than among the relative few who may have fortuitously come to inherit more than others.

Finally, under the heading *Land as a tribal resource*, the Council takes the spirit of the Treaty to its logical conclusion by seeking direct government support for at least some restoration of the Maori people's land base, in recognition, in other words, of the Crown's failure to honour its undertakings of 1840. The corollary is also spelt out: that sale of tribal lands should, as a matter of policy, be 'firmly discouraged'.

So much for some of the principles imbedded in Article 2 of the Treaty and formulated in terms of 'te tino rangatiratanga'. Another may be singled out for a concluding comment. The theme of royal protection

(enshrined in the first part of Article 3) emerged constantly and explicitly in the discussions of the Treaty in 1840 and it is one which has remained in the forefront of Maori thought and expectations ever since. It has inspired deputations of chiefs to the Queen in England in the 19th century, appeals to the Judicial Committee of the Privy Council, and support for the maintenance of a separate body of land law and a court to administer it. It appeared to reach a culmination in the Treaty of Waitangi Act 1975, which established a tribunal that was to make recommendations on claims relating to the practical application of the Treaty and to consider, in terms of the Treaty, such proposed legislation as might be referred to it by Parliament.

In the third chapter of the Council's Kaupapa devoted to servicing organisations such as the Department of Maori Affairs and Maori Land Court, attention is drawn to the role of the above-mentioned tribunal, the Waitangi Tribunal. The Tribunal's membership was made up of the Chief Judge, Maori Land Court, *ex officio* and chairman, and two ministerial appointees, one being a Maori (a membership which has been increased by subsequent amendments). Hence while it is reasonable to regard it as fulfilling a servicing function, that function is of a somewhat different order from the others. The characteristic that sets the Tribunal apart and makes it relevant to the present discussion is its primary role of hearing Maori claims. Prior to 1975 claims had often been lodged by Maori landowners with the courts; at other times they had been the subjects of commissions of enquiry, sometimes the subjects of direct negotiation with government or a semi-public body such as a church. This inherent ambiguity appears not to have been removed by the advent of the Waitangi Tribunal; but the latter has, nevertheless, brought into focus the importance of unsettled land claims to the Maori people and, in theory at least, the importance of the Treaty of Waitangi to the hearing of these claims.

Across the whole spectrum of race relations it would be difficult to imagine a more emotive issue than unsettled land claims by an indigenous minority, or one more dependent for its resolution on the open acceptance of a treaty by the parties bound by it. Yet unlike many other countries where treaties and the facts of prior occupation have been incorporated into domestic law, New Zealand courts and Parliament itself have consistently rejected the Treaty for this purpose. The Council did not, however, favour removing the hiatus by having the Treaty ratified and claims settled in court. Rather, they believed that it should be the 'spirit' of the Treaty that should underpin attempts to settle claims. Furthermore, as a matter of praxis, they held that '...many claims could not be settled solely or adequately on a prescriptive definition of treaty rights as determined through the judicial process' (NZMC 1983:35).

On the other hand, the Council was firmly of the view that while the Tribunal could continue to operate within the parameters of what in effect was a commission of enquiry, its scope for initiative ought to be greatly extended. For instance, it ought to be able to seek out information of its own volition on any claim or any aspect of a claim, from individuals, from organisations, from the courts and, above all, from Parliament itself. By diligent research and competent analysis the Tribunal could inform, recommend or mediate, so helping to remove grievances and promote understanding. In any event, the Tribunal's reports should be tabled in Parliament and otherwise be made public. Virtually all of these prescriptive comments have since (1983) been realised in Tribunal procedure. We may speculate at this point, however, that the recommendatory powers of the Tribunal may come to be superseded in practice by its responsibility to make findings of fact – thereby providing the Treaty partners, Crown and Maori, with a firm, independently formulated basis upon which to negotiate. And negotiating over the rival claims of sovereignty and rangatiratanga was not only at the heart of the Council's Kaupapa, it was also at the threshold of relations between Maori and Pakeha in the final quarter of the 20th century.

NOTES

1. While the focus of our ethnographic interests had little in common, Ralph Bulmer and I most certainly shared a concern to use the insights of social anthropology the better to understand the consequences of attempting to translate the categories of one language-culture system into those of another. This essay (not hitherto published) was written some years ago at a time when there was a resurgence of interest in Maori ethnicity, underpinned by the Treaty of Waitangi of 1840. This Treaty not only established New Zealand as a British Crown colony, it also stands as the first major attempt to juxtapose the categories of thought and action of Maori and European.
I dedicate the essay with respect and affection to 'Raef'.
2. The background to this publication is given in the first paragraph of the Explanatory Note to the Maori Affairs Bill 1983:
'In 1978 the Government introduced a consolidation of the Maori Affairs Act 1953 and the Maori Housing Act 1935, and their various amendments. However, at the request of the Maori people it was decided not to proceed with that Bill, and to invite the New Zealand Maori Council to consider the existing legislation and to make recommendations to the Government for its revision. In February 1983 the Council submitted to the Government *Te Wahanga Tuatahi*. Most of the proposals set out in that Kaupapa have been accepted in principle by the Government and it is proposed to introduce legislation giving effect to them.'
3. cf., for example, a seminar on the Treaty of Waitangi, Victoria University, Wellington, February 1972.

4. Similarly with 'Kuini-mana', the mana of the Queen, a term that soon came into colloquial use which also had no obvious tribal referent.
5. Paora Tuhaere of Ngati Whatua whose political life for 40 years was shaped very largely by the Treaty and its implications said to a conference of 200 chiefs drawn from tribes from all parts of New Zealand at Kohimaramara in 1860 'I timataia te whakakotahitanga ki tena, me te rironga ano hoki o te mana o tenei motu ki raro ki te maru o te Kuini. . . ' (Messenger August 1860: 4). 'The union (of the two races) began with that (the Treaty), together with the placing of the mana of this island (North Island) under the protection of the Queen. . . '. For Tuahere, it was not the 'island' that was placed under the mana of the Queen, but simply chiefs' mana placed under her protection i.e. somewhat akin, perhaps, to a feudal system. (Sovereignty over the South Island was claimed by Hobson by right of Cook's 'discovery' (Wards 1968: 46ff)).
6. Although by the 20th century Ngata (1922:12) was able to say with hindsight that Article 3 ' . . . represents the greatest benefit bestowed upon the Maori people by Her Majesty the Queen.' The New Zealand Maori Council, itself created by statute, would doubtless concur. Their concern in 1983, however, was not to contest the principle, but to make it more effective in practice.
7. cf Williams's and Busby's defence of their acquisition of personal land interests (Colenso 1890: 20f).
8. It is not unreasonable to assume that powerful chiefs would have believed that as proprietors and trustees of tribal estates they would have been free to negotiate sales of land on equal terms with the Crown, and that they would not have been prevented from exercising their chiefly rights to sell on the open market providing they did not infringe the rights of others and so bring about the intervention of the Crown. cf. Ross 1972:146f re complaints of Te Kawau and others. At the time, therefore, pre-emption for most would have been a non-issue.
9. Further to the previous note, it could be asserted that other matters, such as fishing rights, that were eventually to become the subjects of much litigation were not taken up by the chiefs at the time of the Treaty (or later, as at Kohimaramara), not because their omission from the Treaty (Maori version) was of no concern to them, but because a specification of rangatiratanga, which fully covered these matters, was clearly not omitted.
10. cf. NZMC 1983:8. See further below.
11. It should not be thought that Maori idioms referring to the relationship between a chief and the Governor as being that between a parent and a child betokened an attitude of subservience e.g. Hone Heke of Matarahurahu at Waitangi before signing the Treaty saying: 'Governor, sit! a father, a Governor for us' (Colenso 1890:26). It was, rather, an encomium and a plea for assistance e.g. Tamati Waka Nene of Ngatihao, also at Waitangi: 'O Governor! Sit. . . remain for us – a father, a judge, a peace maker.' (Colenso 1890:27); cf. Ward 1978:42 and the idea of securing an ally to keep the settlers and French in check. 20 years later Tamati Waka Nene was to say: 'My object in accepting the Governor was that I might have a protector. . . When the fame of New Zealand became known, the French arrived, and the Americans arrived.' *Messenger*, August 1860:16f.) There was, too, hope for some sort of joint enterprise as between equals e.g. Nopera Panakareao at Kaitaia in 1840: 'The shadow of the land goes to Queen Victoria, but the substance remains to us.' (Wards 1968:vii) Even Hobson himself contributed: 'He iwi tahi tatou' – We are (now) one people – a remark addressed to each signatory in turn at Waitangi and evidently suggesting a prospect more one of collaboration than subjugation (Colenso 1890:35).
12. e.g. the paramount chief of Ngati Tuwharetoa, Te Heuheu II Tukino chastised two lesser chiefs who signed the Treaty without authority with the words: 'What amazing conduct is this of yours? Were you two, indeed, sent to perform such acts? . . . is it for you to place the mana of Te Heuheu beneath the feet of a woman? . . . I will consent to neither your acts nor your goods. As for these blankets (given to those who signed the Treaty), burn them.' (Caselberg 1975:50).
13. cf., for example, Ross 1972:132ff; Ward 1978:43f; Wards 1968:40ff; Laurenson 1972:59f, 234f.
14. cf. footnote 2.

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‘HISTORY IS A WAY OF MUSING UPON OURSELVES’

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One reviewer of Umberto Eco's novel *The Name of the Rose* argued that

Eco has thus decided to write a novel, not simply because what cannot be theorised must be narrated, but because literature can both narrate and theorise, and is thus an ideal situation to carry out and develop the semiotic discourse (Schiavoni 1984:573).

In Eco's hands semiotics is a unifying process in which various kinds and levels of discourse are brought together and into tension with each other, and literature is both an art form and a form of self-critical awareness, a discipline for seeking meaning.

What follows is a reflection on the tensions between people who seek to communicate across cultures. Since it is a fragment from a larger sequence of experiences and encounters by a historian among the mountain dwelling Enga of Papua New Guinea, it is a short story rather than a novel. In this sense it is a piece of fiction. The use of these terms from literature recalls the sceptical though humorous jibes which some of the American missionaries, on the edge of whose station we resided, used to greet me and inquire after my research. One, in particular, would ask how my novel was going, rather than how my research was progressing.

The outcome of that research among the Enga from 1971 to 1973 was a doctoral dissertation which addressed, in a formal way, issues of methodology about the status and interpretation of oral traditions as historical sources (Lacey 1975). Perhaps, if I had been free from the constraints of a doctoral programme, I could have written an intuitive and autobiographical piece of literature akin to Kenneth Read's *The High Valley*. Perhaps, in what follows about the interactions and relationships between Kapenge and me, there are some echoes of Read's vivid portrayal of his relationships with Makis. Elsewhere I have reflected, in a more formal manner, upon my relationships with Kepai (his actual rather than his fictionalised name), and our mutual growth in historical consciousness through the dynamics of the 'conversational narratives' we wove together (Lacey 1980).

James Hillman, whose epigram¹ flies at the masthead of this opening statement, writes of 'healing fictions' and of the ways in which, in our evolving life histories, we have an underlying myth or plot, images stemming from our imaginative story or fiction, which we call our life (O'Connor 1986:12).² And Jerome Bruner points up the reciprocity and tension between telling our life histories and living our lives: 'Narrative imitates life, life imitates narrative'. Bruner continues:

There is no such thing psychologically as 'life itself'. At very least, it is a selective achievement of memory recall; beyond that, recounting one's life is an interpretive feat (1987:13).

So life narratives are not, according to these views, written or told in any kind of flexible, or neutral, vacuum. They are neither detached from the person's deeper structures and experience, nor from the shaping context of their culture.